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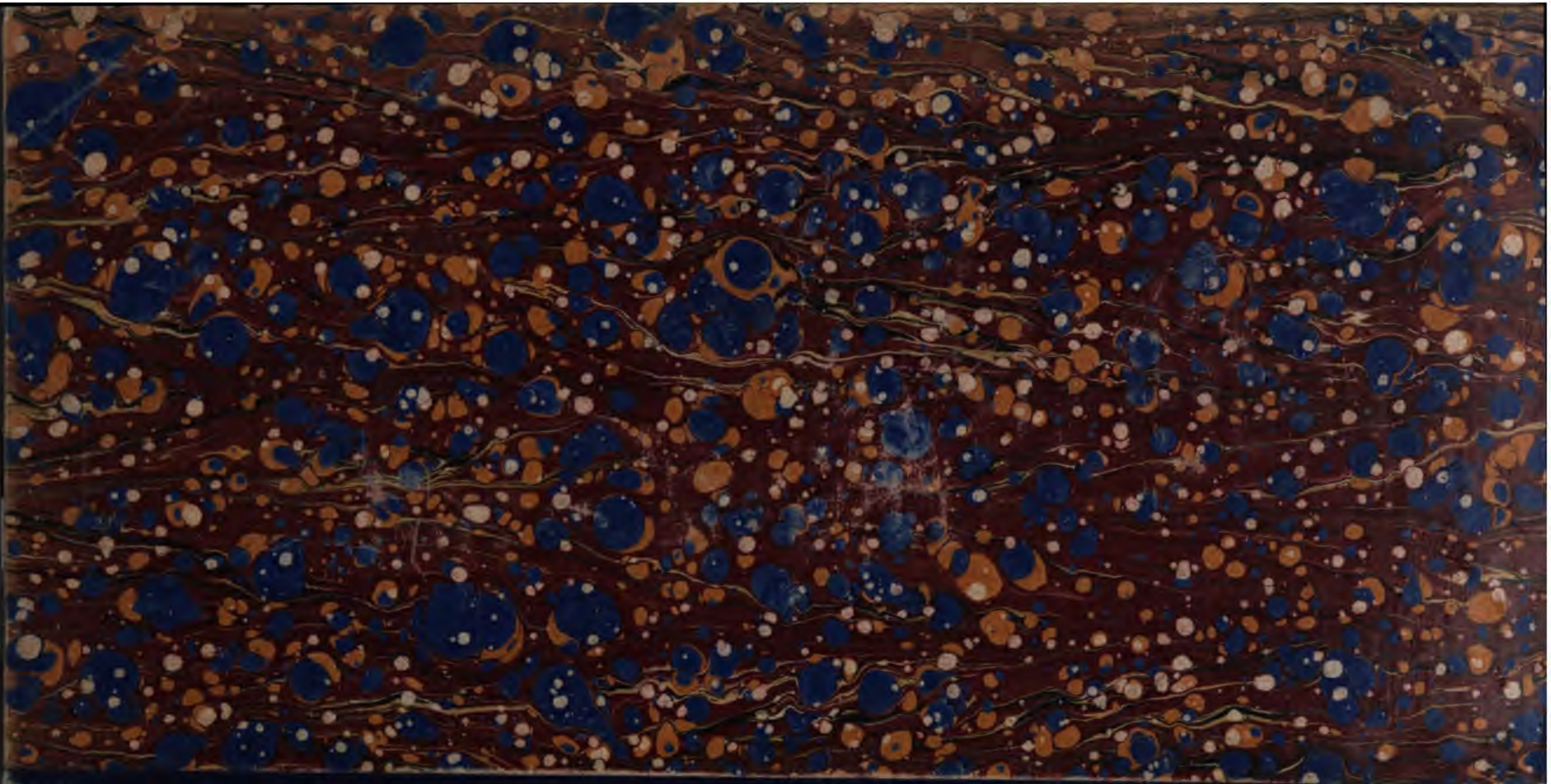

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T. LEWIS, F.R.A.S.,
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The Path of Comet 1902 *b* (Perrine) among the Stars . *Frontispiece*

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ANNUAL COMPANION

TO

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No. 313. 1902. Vol. XXV.

INTRODUCTION.

The present 'Companion' closely resembles that of last year. A list of close Double Stars detected by spectroscopic observation is added for the first time.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of Jupiter V. is from the 'Connaissance de Temps,' 1902.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon, except in the Ephemerides of Variable Stars, where Greenwich Civil Time is used, the day being reckoned from midnight to midnight.

Date.	Day of Year.	Fr. of Year.	Sun		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 5	5	·011	4 4	20 7	22 41 S	0 5 17	18 56 35	Jan.
12	12	·030	4 13	20 4	21 45	8 15	19 24 11	☾ 1 4 8
19	19	·049	4 24	19 57	20 28	10 44	19 51 47	● 9 9 15
26	26	·068	4 36	19 49	18 52	12 36	20 19 22	☽ 16 18 38
								○ 23 12 6
Feb. 2	33	·088	4 49	19 39	17 0	13 50	20 46 58	☾ 31 1 9
9	40	·107	5 2	19 27	14 52	14 24	21 14 34	Feb.
16	47	·126	5 14	19 14	12 33	14 19	21 42 10	● 8 1 21
23	54	·145	5 27	19 0	10 4	13 39	22 9 46	☽ 15 2 57
								○ 22 1 3
Mar. 2	61	·164	5 39	18 45	7 27	12 28	22 37 22	March.
9	68	·183	5 51	18 29	4 45	10 53	23 4 58	☾ 1 22 39
16	75	·203	6 3	18 14	2 0 S	9 0	23 32 34	● 9 14 50
23	82	·222	6 15	17 58	0 46 N	6 56	0 0 9	☽ 16 10 13
30	89	·241	6 27	17 42	3 31	4 47	0 27 45	○ 23 15 21
								☾ 31 18 24
April 6	96	·260	6 38	17 26	6 12	2 41	0 55 21	April.
13	103	·279	6 50	17 11	8 48	0 0 45	1 22 57	● 8 1 50
20	110	·298	7 2	16 56	11 17	23 59 4	1 50 33	☽ 14 17 26
27	117	·318	7 13	16 41	13 37	57 42	2 18 9	○ 22 6 50
								☾ 30 10 58
May 4	124	·337	7 24	16 28	15 46	56 45	2 45 45	May.
11	131	·356	7 36	16 10	17 42	56 16	3 13 21	● 7 10 45
18	138	·375	7 46	16 6	19 24	56 14	3 40 56	☽ 14 1 40
25	145	·394	7 56	15 57	20 50	56 38	4 8 32	○ 21 22 46
								☾ 30 0 0
June 1	152	·413	8 4	15 51	21 58	57 28	4 36 8	June.
8	159	·433	8 11	15 46	22 47	23 58 39	5 3 44	● 5 18 11
15	166	·452	8 16	15 44	23 17	0 0 3	5 31 20	☽ 12 11 54
22	173	·471	8 19	15 45	23 27	1 33	5 58 56	○ 20 14 17
29	180	·490	8 19	15 48	23 17	3 1	6 26 32	☾ 28 9 52
								July.
July 6	187	·509	8 16	15 53	22 46	4 21	6 54 8	● 5 0 59
13	194	·528	8 11	16 0	21 56	5 25	7 21 44	☽ 12 0 47
20	201	·548	8 4	16 8	20 48	6 5	7 49 19	○ 20 4 45
27	208	·567	7 55	16 18	19 23	6 18	8 16 55	☾ 27 17 15
								August.
Aug. 3	215	·586	7 44	16 28	17 42	6 3	8 44 31	● 3 8 17
10	222	·605	7 32	16 39	15 47	5 19	9 12 7	☽ 10 16 24
17	229	·624	7 19	16 50	13 39	4 6	9 39 43	○ 18 18 3
24	236	·643	7 4	17 1	11 21	2 27	10 7 19	☾ 25 23 5
31	243	·663	6 49	17 12	8 54	0 0 28	10 34 55	
								Sept.
Sept. 7	250	·682	6 34	17 23	6 20	23 58 14	11 2 31	● 1 17 19
14	257	·701	6 18	17 35	3 41	55 49	11 30 6	☽ 9 10 15
21	264	·720	6 2	17 46	0 58 N	53 20	11 57 42	○ 17 6 23
28	271	·739	5 45	17 57	1 45 S	50 55	12 25 18	☾ 24 4 32
								Oct.
Oct. 5	278	·758	5 29	18 9	4 29	48 41	12 52 54	● 1 5 9
12	285	·778	5 14	18 20	7 9	46 44	13 20 30	☽ 9 5 21
19	292	·797	4 59	18 32	9 45	45 10	13 48 6	○ 16 18 1
26	299	·816	4 45	18 45	12 13	44 7	14 15 42	☾ 23 10 58
								● 30 20 14
Nov. 2	306	·835	4 31	18 57	14 33	43 41	14 43 17	Nov.
9	313	·854	4 19	19 10	16 41	43 52	15 10 53	☽ 8 0 31
16	320	·873	4 9	19 22	18 35	44 45	15 38 29	○ 15 5 7
23	327	·893	4 0	19 33	20 13	46 18	16 6 5	☾ 21 19 47
30	334	·912	3 54	19 44	21 33	48 31	16 33 41	● 29 14 4
								Dec.
Dec. 7	341	·931	3 50	19 53	22 33	51 17	17 1 17	☽ 7 18 27
14	348	·950	3 49	20 1	23 11	54 27	17 28 53	○ 14 15 47
21	*355*	*·969*	3 51	20 5	23 27	23 57 52	17 56 29	☾ 21 8 0
28	*362*	*·988*	3 55	20 8	23 19	0 1 21	18 24 5	● 29 9 25

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.
		h m	h m	° ′	°
1	W	12 49	18 17	7 49 S	+ 1·7 E.
2	Th	13 54	19 1	11 26	−10·5 E.
3	F	14 57	19 46	14 32	−22·8 E.
4	S	15 58	20 31	17 1	−35·0 E.
5	**S**	16 56	21 19	18 48	−47·1 E.
6	M	17 49	22 7	19 46	−59·3 E.
7	Tu	18 37	22 56	19 51	−71·5 E.
8	W	19 20	23 45	19 1	−83·6 E.
9	Th	sets	......	17 17	+84·6 M.
10	F	5 21	0 34	14 42	+72·3 M.
11	S	6 28	1 23	11 23	+60·1 M.
12	**S**	7 37	2 11	7 29	+47·9 M.
13	M	8 49	2 58	3 11 S	+35·8 M.
14	Tu	10 2	3 46	1 20 N	+23·6 M.
15	W	11 16	4 34	5 52	+11·5 M.
16	Th	12 29	5 24	10 10	− 0·7 M.
17	F	13 46	6 17	13 58	−12·9 M.
18	S	15 2	7 12	17 1	−25·0 M.
19	**S**	16 13	8 10	19 3	−37·2 M.
20	M	17 17	9 10	19 51	−49·4 M.
21	Tu	18 11	10 44	19 19	−61·5 M.
22	W	18 55	11 10	17 31	−73·6 M.
23	Th	rises	12 7	14 39	−85·8 M.
24	F	5 46	13 1	10 57	+82·1 E.
25	S	7 1	13 51	6 46	+69·9 E.
26	**S**	8 14	14 39	2 22 N	+57·7 E.
27	M	9 25	15 26	2 1 S	+45·6 E.
28	Tu	10 33	16 11	6 11	+33·4 E.
29	W	11 39	16 55	10 0	+21·2 E.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.
		h m	h m	° ′	°
1	S	14 45	19 12	18 5 S	−15·2 E.
2	**S**	15 49	20 0	19 21	−27·4 E.
3	M	16 31	20 49	19 45	−39·6 E.
4	Tu	17 14	21 38	19 16	−51·7 E.
5	W	17 54	22 27	17 52	−63·9 E.
6	Th	18 28	23 16	15 35	−76·1 E.
7	F	sets	......	12 31	−88·2 E.
8	S	5 24	0 5	8 46	+79·6 M.
9	**S**	6 35	0 54	4 31 S	+67·4 M.
10	M	7 50	1 43	0 1 N	+55·2 M.
11	Tu	9 4	2 32	4 36	+43·1 M.
12	W	10 21	3 22	9 0	+30·9 M.
13	Th	11 35	4 14	12 56	+18·7 M.
14	F	12 50	5 8	16 8	+ 6·6 M.
15	S	14 0	6 4	18 24	− 5·6 M.
16	**S**	15 6	7 2	19 32	−17·8 M.
17	M	16 3	8 1	19 26	−29·9 M.
18	Tu	16 49	8 59	18 7	−42·1 M.
19	W	17 28	9 55	15 42	−54·3 M.
20	Th	18 1	10 49	12 24	−66·5 M.
21	F	rises	11 40	8 29	−78·6 M.
22	S	5 51	12 29	4 13 N	+89·3 E.
23	**S**	7 3	13 16	0 11 S	+77·1 E.
24	M	8 14	14 2	4 27	+64·9 E.
25	Tu	9 20	14 48	8 26	+52·8 E.
26	W	10 27	15 33	11 58	+40·6 E.
27	Th	11 30	16 19	14 55	+28·4 E.
28	F	12 32	17 5	17 13 S	+16·2 E.

MARCH.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.
		h m	h m	° ′	°
1	S	13 28	17 52	18 45 S	+ 4·1 E.
2	**S**	14 20	18 40	19 28	− 8·1 E.
3	M	15 7	19 29	19 19	−20·3 E.
4	Tu	15 48	20 17	18 17	−32·5 E.
5	W	16 23	21 6	16 22	−44·6 E.
6	Th	16 57	21 55	13 38	−56·8 E.
7	F	17 24	22 44	10 10	−69·0 E.
8	S	17 51	23 3	6 6	−81·2 E.
9	**S**	sets	......	1 37 S	+86·6 M.
10	M	6 46	0 24	3 2 N	+74·4 M.
11	Tu	8 4	1 15	7 36	+62·2 M.
12	W	9 22	2 8	11 45	+50·1 M.
13	Th	10 37	3 3	15 14	+37·9 M.
14	F	11 52	4 0	17 46	+25·7 M.
15	S	12 59	4 58	19 10	+13·5 M.
16	**S**	13 57	5 56	19 21	+ 1·4 M.
17	M	14 47	6 53	18 20	−10·8 M.
18	Tu	15 29	7 49	16 15	−23·0 M.
19	W	16 1	8 43	13 16	−35·1 M.
20	Th	16 32	9 34	9 37	−47·3 M.
21	F	16 57	10 22	5 33	−59·5 M.
22	S	17 21	11 10	1 17 N	−71·7 M.
23	**S**	rises	11 56	2 58 S	−83·9 M.
24	M	7 6	12 41	7 1	+83·9 E.
25	Tu	8 11	13 27	10 42	+71·7 E.
26	W	9 17	14 12	13 51	+59·5 E.
27	Th	10 19	14 59	16 23	+47·4 E.
28	F	11 17	15 46	18 10	+35·2 E.
29	S	12 11	16 33	19 9	+23·0 E.

APRIL.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.
		h m	h m	° ′	°
1	Tu	14 19	18 57	17 0 S	−13·6 E.
2	W	14 53	19 45	14 37	−25·8 E.
3	Th	15 22	20 33	11 30	−38·0 E.
4	F	15 50	21 22	7 43	−50·2 E.
5	S	16 15	22 11	3 27 S	−62·4 E.
6	**S**	16 40	23 2	1 8 N	−74·6 E.
7	M	17 7	23 55	5 47	−86·8 E.
8	Tu	sets	......	10 12	+81·1 M.
9	W	8 16	0 51	14 2	+68·9 M.
10	Th	9 34	1 48	16 58	+56·7 M.
11	F	10 46	2 48	18 46	+44·5 M.
12	S	11 52	3 48	19 18	+32·3 M.
13	**S**	12 44	4 48	18 34	+20·1 M.
14	M	13 30	5 45	16 42	+ 7·9 M.
15	Tu	14 4	6 39	13 54	− 4·3 M.
16	W	14 35	7 31	10 25	−16·5 M.
17	Th	15 1	8 20	6 29	−28·7 M.
18	F	15 25	9 7	2 20 N	−40·9 M.
19	S	15 49	9 52	1 52 S	−53·1 M.
20	**S**	16 13	10 37	5 55	−65·3 M.
21	M	16 38	11 22	9 40	−77·5 M.
22	Tu	rises	12 8	12 58	−89·7 M.
23	W	8 8	12 54	15 40	+78·1 E.
24	Th	9 8	13 40	17 41	+65·9 E.
25	F	10 3	14 28	18 54	+53·7 E.
26	S	10 53	15 15	19 18	+41·5 E.
27	**S**	11 38	16 3	18 50	+29·3 E.
28	M	12 18	16 50	17 32	+17·0 E.
29	Tu	12 52	17 38	15 27	+ 4·8 E.

MAY.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	13 49	19 11	9 9 S	−19·6 E.
2	F	14 15	19 59	5 9	−31·8 E.
3	S	14 39	20 48	0 46 S	−44·0 E.
4	**S**	15 6	21 40	3 48 N	−56·2 E.
5	M	15 34	22 33	8 18	−68·4 E.
6	Tu	16 8	23 31	12 26	−80·6 E.
7	W	sets	......	15 51	+87·2 M.
8	Th	8 25	0 31	18 12	+75·0 M.
9	F	9 36	1 33	19 16	+62·8 M.
10	S	10 36	2 35	18 57	+50·6 M.
11	**S**	11 26	3 35	17 23	+38·4 M.
12	M	12 5	4 33	14 45	+26·2 M.
13	Tu	12 38	5 27	11 22	+13·9 M.
14	W	13 6	6 17	7 28	+ 1·7 M.
15	Th	13 32	7 5	3 19 N	−10·5 M.
16	F	13 55	7 51	0 53 S	−22·7 M.
17	S	14 17	8 36	4 58	−34·9 M.
18	**S**	14 42	9 20	8 47	−47·1 M.
19	M	15 8	10 5	12 10	−59·4 M.
20	Tu	15 37	10 51	15 2	−71·6 M.
21	W	16 10	11 37	17 14	−83·8 M.
22	Th	rises	12 24	18 41	+84·0 E.
23	F	8 50	13 12	19 19	+71·8 E.
24	S	9 37	13 59	19 6	+59·6 E.
25	**S**	10 18	14 47	18 2	+47·3 E.
26	M	10 54	15 34	16 10	+35·1 E.
27	Tu	11 25	16 20	13 34	+22·9 E.
28	W	11 53	17 6	10 20	+10·7 E.
29	Th	12 18	17 52	6 34	− 1·5 E.
30	F	12 42	18 39	2 25 S	−13·7 E.
31	S	13 6	19 28	2 0 N	−26·0 E.

JUNE.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	13 32	20 19	6 27 N	−38·2 E.
2	M	14 4	21 13	10 41	−50·5 E.
3	Tu	14 37	22 11	14 25	−62·7 E.
4	W	15 21	23 12	17 17	−74·9 E.
5	Th	sets	......	18 59	−87·1 E.
6	F	8 19	0 15	19 18	+80·7 M.
7	S	9 15	1 18	18 14	+68·5 M.
8	**S**	10 1	2 19	15 57	+56·3 M.
9	M	10 38	3 16	12 42	+44·0 M.
10	Tu	11 10	4 10	8 50	+31·8 M.
11	W	11 35	5 1	4 38	+19·6 M.
12	Th	12 0	5 48	0 20 N	+ 7·4 M
13	F	12 24	6 34	3 51 S	− 4·8 M.
14	S	12 47	7 19	7 46	−17·0 M.
15	**S**	13 12	8 4	11 18	−29·2 M.
16	M	13 40	8 49	14 19	−41·4 M.
17	Tu	14 11	9 35	16 42	−53·6 M.
18	W	14 48	10 21	18 23	−65·9 M.
19	Th	15 31	11 9	19 15	−78·1 M.
20	F	rises	11 56	19 17	+89·6 E.
21	S	8 19	12 44	18 27	+77·4 E.
22	**S**	8 56	13 32	16 48	+65·2 E.
23	M	9 30	14 18	14 23	+53·0 E.
24	Tu	9 57	15 4	11 19	+40·7 E.
25	W	10 24	15 50	7 43	+28·5 E.
26	Th	10 48	16 36	3 43 S	+16·3 E.
27	F	11 11	17 23	0 33 N	+ 4·1 E.
28	S	11 36	18 11	4 54	− 8·2 E.
29	**S**	12 3	19 2	9 7	−20·4 E.
30	M	12 35	19 56	12 57 N.	−32·6 E.

JULY.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	13 11	20 54	16 8 N	−44·8 E.
2	W	13 58	21 55	18 20	−57·0 E.
3	Th	14 54	22 57	19 18	−69·2 E.
4	F	16 2	23 59	18 54	−81·5 E.
5	S	sets	......	17 10	+86·3 M.
6	**S**	8 33	1 0	14 17	+74·1 M.
7	M	9 7	1 57	10 35	+61·9 M.
8	Tu	9 37	2 50	6 23	+49·6 M.
9	W	10 3	3 41	2 0 N	+37·4 M.
10	Th	10 28	4 29	2 21 S	+25·2 M.
11	F	10 52	5 15	6 27	+13·0 M.
12	S	11 17	6 1	10 10	+ 0·7 M.
13	**S**	11 44	6 46	13 22	−11·5 M.
14	M	12 14	7 32	15 58	−23·7 M.
15	Tu	12 49	8 18	17 52	−35·9 M.
16	W	13 30	9 5	19 0	−48·2 M.
17	Th	14 15	9 53	19 17	−60·4 M.
18	F	15 7	10 41	18 43	−72·6 M.
19	S	16 4	11 29	17 19	−84·8 M.
20	**S**	rises	12 16	15 7	+82·9 E.
21	M	8 3	13 3	12 14	+70·7 E.
22	Tu	8 30	13 49	8 45	+58·9 E.
23	W	8 55	14 35	4 50	+46·2 E.
24	Th	9 18	15 21	0 39 S	+34·0 E.
25	F	9 42	16 9	3 39 N	+21·8 E.
26	S	10 9	16 58	7 51	+ 9·6 E.
27	**S**	10 37	17 49	11 44	− 2·6 E.
28	M	11 10	18 44	15 3	−14·8 E.
29	Tu	11 51	19 41	17 32	−27·0 E.
30	W	12 41	20 41	18 57	−39·3 E.
31	Th	13 40	21 42	19 7 N	−51·5 E.

AUGUST.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	14 51	22 42	17 59 N	−61·7 E.
2	S	16 7	23 41	15 38	−75·9 E.
3	**S**	sets	......	12 18	−88·1 E.
4	M	7 38	0 36	8 18	+79·6 M
5	Tu	8 5	1 29	3 56 N	+67·4 M.
6	W	8 32	2 19	0 31 S	+55·2 M.
7	Th	8 55	3 7	4 47	+43·0 M.
8	F	9 21	3 54	8 43	+30·8 M.
9	S	9 47	4 40	12 10	+18·6 M.
10	**S**	10 15	5 27	15 0	+ 6·3 M.
11	M	10 50	6 13	17 9	− 5·9 M.
12	Tu	11 27	7 0	18 32	−18·1 M.
13	W	12 10	7 48	19 6	−30·3 M.
14	Th	13 2	8 35	18 50	−42·5 M.
15	F	13 57	9 23	17 43	−54·7 M.
16	S	14 56	10 11	15 47	−66·9 M.
17	**S**	16 2	10 58	13 7	−79·1 M.
18	M	17 8	11 45	9 48	+88·7 E.
19	Tu	rises	12 32	5 59	+76·4 E.
20	W	7 24	13 19	1 50 S	+64·2 E.
21	Th	7 47	14 7	2 29 N	+52·0 E.
22	F	8 14	14 56	6 44	+39·8 E.
23	S	8 42	15 47	10 41	+27·6 E.
24	**S**	9 14	16 40	14 7	+15·4 E.
25	M	9 52	17 35	16 48	+ 3·2 E.
26	Tu	10 37	18 33	18 28	− 9·2 E.
27	W	11 32	19 31	19 1	−21·4 E.
28	Th	12 36	20 30	18 19	−33·6 E.
29	F	13 47	21 28	16 28	−45·8 E.
30	S	15 1	22 23	13 35	−58·0 E.

SEPTEMBER.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	17 32	......	5 44 N	−82·3 E.
2	Tu	sets	0 8	1 20 N	+85·5 M.
3	W	6 56	0 57	3 1 S	+73·3 M.
4	Th	7 21	1 45	7 8	+61·1 M.
5	F	7 49	2 32	10 48	+48·9 M.
6	S	8 17	3 19	13 53	+36·6 M.
7	**S**	8 49	4 6	16 18	+24·4 M.
8	M	9 25	4 53	17 57	+12·2 M.
9	Tu	10 6	5 41	18 48	+ 0·0 M.
10	W	10 54	6 28	18 49	−12·2 M.
11	Th	11 47	7 16	18 0	−24·4 M.
12	F	12 44	8 4	16 23	−36·6 M.
13	S	13 46	8 51	14 0	−48·8 M.
14	**S**	14 51	9 38	10 55	−61·0 M.
15	M	16 1	10 25	7 17	−73·2 M.
16	Tu	17 10	11 13	3 12 S	−85·4 M.
17	W	rises	12 1	1 7 N	+82·4 E.
18	Th	6 17	12 51	5 27	+70·3 E.
19	F	6 45	13 42	9 34	+58·0 E.
20	S	7 17	14 35	13 13	+45·8 E.
21	**S**	7 52	15 31	16 6	+33·6 E.
22	M	8 37	16 28	18 2	+21·4 E.
23	Tu	9 29	17 26	18 50	+ 9·2 E.
24	W	10 30	18 24	18 26	− 3·0 E.
25	Th	11 36	19 21	16 54	−15·2 E.
26	F	12 46	20 16	14 22	−27·4 E.
27	S	14 1	21 8	11 0	−39·6 E.
28	**S**	15 15	21 59	7 5	−51·8 E.
29	M	16 27	22 48	2 50 N	−64·0 E.

OCTOBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	5 22	......	5 39 S	−88·3 E.
2	Th	5 49	0 24	9 28	+79·5 M.
3	F	6 16	1 11	12 46	+67·3 M.
4	S	6 48	1 58	15 26	+55·1 M.
5	**S**	7 22	2 46	17 22	+42·9 M.
6	M	8 2	3 33	18 30	+30·7 M.
7	Tu	8 47	4 21	18 48	+18·5 M
8	W	9 37	5 9	18 16	+ 6·3 M.
9	Th	10 32	5 56	16 56	− 5·9 M.
10	F	11 32	6 43	14 50	−18·0 M.
11	S	12 36	7 29	12 3	−30·2 M.
12	**S**	13 42	8 16	8 40	−42·4 M.
13	M	14 50	9 2	4 47	−54·6 M.
14	Tu	16 2	9 50	0 34 S	−66·8 M.
15	W	17 16	10 40	3 49 N	−79·0 M.
16	Th	18 32	11 31	8 6	+88·8 E.
17	F	rises	12 25	12 0	+76·7 E.
18	S	5 51	13 21	15 15	+64·5 E.
19	**S**	6 33	14 20	17 33	+52·3 E.
20	M	7 23	15 20	18 41	+40·1 E.
21	Tu	8 23	16 19	18 35	+27·9 E.
22	W	9 28	17 17	17 18	+15·7 E.
23	Th	10 39	18 12	14 58	+ 3·6 E.
24	F	11 52	19 5	11 48	− 8·6 E.
25	S	13 5	19 55	8 3	−20·8 E.
26	**S**	14 15	20 44	3 57 N	−33·0 E.
27	M	15 27	21 31	0 17 S	−45·2 E.
28	Tu	16 37	22 18	4 26	−57·3 E.
29	W	17 44	23 5	8 20	−69·5 E.

NOVEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	5 21	0 39	16 50 S	+74·0 M.
2	**S**	5 57	1 27	18 14	+61·8 M.
3	M	6 40	2 15	18 48	+49·6 M.
4	Tu	7 29	3 2	18 32	+37·4 M.
5	W	8 22	3 49	17 28	+25·2 M.
6	Th	9 19	4 36	15 38	+13·0 M.
7	F	10 20	5 22	13 7	+ 1·0 M.
8	S	11 24	6 7	10 0	−11·2 M.
9	**S**	12 3:	6 53	6 22	−23·4 M.
10	M	13 38	7 39	2 21 S	−35·6 M.
11	Tu	14 51	8 27	1 55 N	−47·7 M.
12	W	16 5	9 16	6 14	−59·9 M.
13	Th	17 22	10 9	10 21	−72·1 M.
14	F	18 38	11 5	13 58	−84·2 M.
15	S	rises	12 3	16 46	+83·6 E.
16	**S**	5 11	13 5	18 26	+71·4 E.
17	M	6 7	14 7	18 49	+59·2 E.
18	Tu	7 14	15 8	17 53	+47·1 E.
19	W	8 25	16 6	15 47	+34·9 E.
20	Th	9 41	17 1	12 44	+22·8 E.
21	F	10 54	17 53	9 3	+10·6 E.
22	S	12 7	18 42	4 58	− 1·6 E.
23	**S**	13 17	19 30	0 45 N	−13·8 E.
24	M	14 28	20 16	3 25 S	−25·9 E
25	Tu	15 34	21 2	7 21	−38·1 E.
26	W	16 41	21 48	10 54	−50·2 E.
27	Th	17 45	22 35	13 56	−62·4 E.
28	F	18 46	23 22	16 19	−74·5 E.
29	S	sets	......	17 57	−86·7 E.

DECEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	5 23	0 58	18 47 S	+69·0 M.
2	Tu	6 15	1 45	17 58	+56·8 M.
3	W	7 9	2 32	16 22	+44·6 M.
4	Th	8 9	3 18	14 4	+32·5 M.
5	F	9 11	4 3	11 10	+20·3 M.
6	S	10 16	4 48	7 45	+ 8·2 M.
7	**S**	11 21	5 32	3 56 S	− 4·0 M.
8	M	12 30	6 18	0 9 N	−16·2 M.
9	Tu	13 40	7 5	4 21	−28·3 M.
10	W	14 53	7 54	8 29	−40·5 M.
11	Th	16 8	8 47	12 19	−52·6 M.
12	F	17 25	9 43	15 31	−64·8 M.
13	S	18 38	10 43	17 47	−77·0 M.
14	**S**	19 47	11 45	18 51	−89·1 M.
15	M	rises	12 49	18 32	+78·7 E.
16	Tu	6 1	13 51	16 53	+66·6 E.
17	W	7 18	14 50	14 6	+54·4 E.
18	Th	8 36	15 45	10 30	+42·2 E.
19	F	9 52	16 38	6 23	+30·0 E.
20	S	11 4	17 27	2 4 N	+17·9 E.
21	**S**	12 18	18 14	2 13 S	+ 5·8 E.
22	M	13 26	19 1	6 16	− 6·3 E
23	Tu	14 32	19 47	9 57	−18·5 E.
24	W	15 37	20 33	13 8	−30·7 E.
25	Th	16 39	21 20	15 42	−42·8 E.
26	F	17 37	22 7	17 33	−55·0 E.
27	S	18 30	22 54	18 38	−67·2 E.
28	**S**	19 17	23 42	18 54	−79·3 E.
29	M	sets	......	18 19	+88·5 M.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	a	δ			*a*	δ	
	°	°			°	°	
Jan. 2-3. .	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−11**	Slow; long.
3 ...	156	+41	Swift.	July-Sept..	335	+73	Swift; short.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks.	10-12	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	10-21	350	−14	Rather slow.
29 ...	213	+52	Very swift.	14 ...	311	+62	Rather swift.
Feb. 5-10	74	+43	Slow; bright.	15 ...	290	+53	Swift; bright.
15 ...	236	+11	Swift; streaks.	21-25	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Aug.-Sept..	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	Sept. 5-15	62	+37	Swift; streaks.
Mar. 1-4...	166	+ 4	Slow; bright.	6-17	106	+52	Swift; streaks.
14 ...	250	+54	Swift.	10-22	74	+42	Swift; streaks.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow fireballs.	Oct. 2 ...	230	+52	Slow; bright.
17-24	241	+47	Small; short.	4 ...	133	+79	Swift; streaks.
18-23	189	−31	Slow; long.	4 ...	310	+77	Slowish.
20-21	261	+36	Swift; bl. white.	8 ...	77	+31	Swift; streaks.
20-21	**270**	**+33**	Swift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	16-23	89	+ 8	Swift; streaks.
May 1 ...	239	+46	Small; short.	18-20	**92**	**+15**	Swift; streaks.
5-17	254	−21	Slowish.	23 ...	99	+13	Swift; streaks.
6 ...	**338**	**− 2**	Swift; streaks.	29 ...	109	+23	Very swift.
7 ...	246	+ 3	Slow; bright.	Nov. 1 ...	43	+22	Slow; bright.
11-18	231	+27	Slow; small.	2 ...	55	+ 9	Slow; bright.
29 ...	264	+64	Slowish.	10-12	133	+31	Very swift; streaks.
30 ...	333	+27	Swift; streaks.	14-16	**150**	**+22**	Swift; streaks.
June-July.	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
June 10 ...	261	+ 5	Very slow.	20-23	63	+23	Slow; bright.
11-19	274	+69	Rather swift.	23-24	**25**	**+43**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-28	208	+43	Very swift.
20 ...	335	+57	Swift.	30 ...	190	+58	Swift; streaks.
26 ...	354	+39	Swift; streaks.	Dec. 4 ...	162	+58	Swift; streaks.
July 4 ...	303	+24	Swift.	6 ...	80	+23	Slow; bright.
7-Aug.	333	+27	Swift.	8 ...	145	+ 7	Swift; streaks.
6-12	282	−13	Very slow.	8 ...	208	+71	Rather swift.
15-31	23	+43	Swift; streaks.	10-12	**108**	**+33**	Swift; short.
19 ...	314	+48	Swift; short.	12 ...	119	+29	Rather swift.
19-24	290	+24	Slow.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November may be somewhat plentiful in 1902 and 1903.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	a	δ		*a*	δ		*a*	δ
	°	°		°	°		°	°
July 20 ...	19·7	+50·9	July 30 ...	30·8	+53·8	Aug. 9 ...	42·6	+56·5
22 ...	21·8	+51·6	Aug. 1 ...	33·2	+54·4	11 ...	45·0	+57·0
24 ...	24·0	+52·2	3 ...	35·5	+55·0	13 ...	47·5	+57·5
26 ...	26·2	+52·8	5 ...	37·9	+55·5	15 ...	50·1	+58·0
28 ...	28·5	+53·3	7 ...	40·2	+56·0	17 ...	52·6	+58·5

1902.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	18 42 51	24 50 S.	4·6	20 18	0 2	3 46	21 45 10	13 47 S.	35·6	22 12	3 4	7 56	20 8 8	21 20 S.	4·2	21 21	1 27	5 33
11...	19 54 11	23 0	4·8	20 38	0 34	4 30	22 5 53	10 11	41·6	21 34	2 45	7 56	20 40 45	19 29	4·0	21 2	1 20	5 38
21...	21 3 57	18 36	5·4	20 41	1 4	5 27	22 15 1	7 5	48·8	20 48	2 15	7 42	21 12 42	17 17	4·0	20 41	1 13	5 51
31...	22 3 8	12 14	6·4	20 24	1 24	6 24	22 9 27	5 5	56·2	19 53	1 30	7 7	21 43 55	14 47	4·0	20 19	1 5	5 51
Feb. 10...	22 25 33	7 20	8·8	19 40	1 7	6 34	21 50 5	4 45	61·0	18 52	0 31	6 10	22 14 24	12 1	4·0	19 55	0 56	5 57
20...	21 53 46	8 51	10·4	18 29	23 48	5 7	21 26 24	6 2	60·2	17 51	23 23	4 55	22 44 13	9 4	4·0	19 29	0 46	6 3
Mar. 2...	21 29 12	12 40	9·6	17 50	22 48	3 46	21 12 13	7 56	54·2	17 8	22 31	3 54	23 13 26	6 0	4·0	19 3	0 36	6 9
12...	21 45 15	13 38	8·0	17 35	22 28	3 21	21 13 56	9 25	46·4	16 39	21 54	3 9	23 42 12	2 51 S.	4·0	18 36	0 25	6 14
22...	22 25 17	11 38	6·8	17 26	22 30	3 34	21 29 33	10 0	39·4	16 20	21 32	2 44	0 10 39	0 19 N.	4·0	18 9	0 14	6 19
Apr. 1...	23 16 57	7 13	6·0	17 16	22 43	4 10	21 54 56	9 33	33·8	16 3	21 18	2 33	0 38 54	3 27	3·8	17 42	0 3	6 24
11...	0 16 15	0 46 S.	5·4	17 3	23 3	5 2	22 26 31	8 8	29·2	15 49	21 11	2 33	1 7 6	6 30	3·8	17 15	23 51	6 27
21...	1 23 55	7 17 N.	5·0	16 52	23 33	6 14	23 1 46	5 52	25·8	15 33	21 7	2 41	1 35 22	9 25	3·8	16 48	23 40	6 32
May 1...	2 42 21	15 56	5·0	16 38	0 8	7 36	23 39 17	2 56 S.	23·0	15 16	21 5	2 54	2 3 47	12 10	3·8	16 22	23 29	6 36
11...	4 6 20	22 40	5·6	16 39	0 53	9 7	0 18 18	0 29 N.	20·8	15 0	21 5	3 10	2 32 28	14 41	3·8	15 58	23 18	6 38
21...	5 19 7	25 27	6·8	16 54	1 26	9 58	0 58 30	4 13	18·8	14 42	21 6	3 30	3 1 25	16 58	3·8	15 32	23 8	6 42
31...	6 8 9	24 57	8·6	17 8	1 36	10 4	1 39 58	8 3	17·4	14 23	21 8	3 53	3 30 41	18 58	3·8	15 11	22 57	6 43
June 10...	6 26 33	22 39	10·6	17 5	1 15	9 25	2 22 55	11 48	16·0	14 7	21 12	4 17	4 0 14	20 39	3·8	14 52	22 48	6 44
20...	6 13 59	20 3	12·0	16 39	0 23	8 15	3 7 34	15 16	15·0	13 52	21 17	4 42	4 29 58	21 59	3·8	14 32	22 38	6 44
30...	5 51 49	18 44	11·2	15 32	23 16	7 0	3 54 9	18 16	14·2	13 41	21 24	5 7	4 59 49	22 59	3·8	14 15	22 28	6 41
July 10...	5 52 59	19 41	9·0	14 50	22 41	6 31	4 42 41	20 35	13·4	13 36	21 34	5 32	5 29 39	23 38	4·0	14 1	22 19	6 37
20...	6 30 21	21 40	7·0	14 38	22 43	6 48	5 32 55	22 3	12·8	13 38	21 45	5 52	5 59 18	23 56	4·0	13 49	22 9	6 29
30...	7 41 1	21 59	5·6	15 8	23 16	7 24	6 24 18	22 33	12·2	13 47	21 57	6 7	6 28 37	23 52	4·0	13 39	21 59	6 19
Aug. 9...	9 6 25	18 21	5·0	......	......	......	7 16 7	22 0	11·8	14 2	22 9	6 16	6 57 27	23 29	4·0	13 31	21 48	6 5
19...	10 23 44	11 43	5·0	17 30	0 36	7 42	8 7 32	20 24	11·4	14 25	22 21	6 17	7 25 40	22 48	4·2	13 25	21 37	5 49
29...	11 28 13	4 9 N.	5·2	18 36	1 1	7 26	8 57 55	17 50	11·0	14 52	22 32	6 12	7 53 10	21 49	4·2	13 20	21 25	5 30
Sept. 8...	12 23 7	3 9 S.	5·6	19 28	1 17	7 5	9 46 56	14 25	10·8	15 21	22 41	6 1	8 19 53	20 36	4·2	13 15	21 12	5 9
18...	13 10 24	9 35	6·2	20 10	1 25	6 40	10 34 32	10 19	10·6	15 52	22 49	5 46	8 45 46	19 10	4·4	13 12	20 59	4 46
28...	13 47 44	14 26	7·2	20 34	1 22	6 10	11 21 2	5 44	10·4	16 23	22 56	5 29	9 10 46	17 33	4·6	13 7	20 44	4 22
Oct. 8...	14 3 27	16 14	8·8	20 21	0 58	5 35	12 6 54	0 51 N.	10·2	16 55	23 3	5 11	9 34 55	15 48	4·6	13 2	20 29	3 56
18...	13 38 3	12 2	10·0	18 46	23 46	4 46	12 52 44	4 7 S.	10·0	17 26	23 9	4 52	9 58 11	13 56	4·8	12 57	20 13	3 29
28...	13 11 13	6 4	8·4	17 11	22 44	4 17	13 39 12	8 58	10·0	17 58	23 16	4 34	10 20 37	11 59	5·0	12 50	19 56	3 2
Nov. 7...	13 39 2	7 55	6·2	17 13	22 37	4 1	14 26 57	13 29	9·8	18 31	23 25	4 19	10 42 10	10 1	5·2	12 44	19 38	2 32
17...	14 33 21	13 28	5·2	17 59	22 53	3 47	15 16 27	17 27	9·8	19 5	23 35	4 5	11 2 51	8 2	5·6	12 35	19 19	2 3
27...	15 34 57	18 52	4·8	18 53	23 15	3 37	16 7 57	20 38	9·8	19 36	23 47	3 58	11 22 37	6 6	5·8	12 25	18 59	1 33
Dec. 7...	16 40 12	22 55	4·6	19 45	23 42	3 39	17 1 19	22 51	9·8	20 4	0 0	3 56	11 41 24	4 14	6·2	12 14	18 38	1 2
17...	17 48 38	25 3	4·6	20 26	0 8	3 50	17 55 59	23 56	9·8	20 26	0 15	4 2	11 59 3	2 28	6·6	12 1	18 17	0 33
27...	18 59 10	24 53 S.	4·8	20 56	0 39	4 22	18 51 1	23 46 S.	10·0	20 41	0 31	4 21	12 15 25	0 51 N.	7·2	11 46	17 53	0 0

Mercury.

	d h		d h		d h
Gr. elong. E. Feb.	3 0;	May	28 6;	Sept.	24 17
" W. Mar.	17 2;	July	15 15;	Nov.	4 1
Sup. conj. ...Jan.	1 18;	Apr.	28 13;	Aug.	11 3
				Dec.	12 0
Inf. conj. ...Feb.	18 9;	June	23 8;	Oct.	19 7
Stationary...Feb.	8 20;	Mar.	2 14;	June	10 13

Venus.

	d h		d h
Gr. elong. W....Apr.	25 12		
Sup. conj. ......Nov.	28 14		
Inf. conj.........Feb.	14 11		
Stationary ......Jan.	22 10;	Mar.	5 21
Gr. brilliancy ..Jan.	9 15;	Mar.	20 19

Superior Planets.

	Conjunction. d h	Opposition. d h	Quadrature. d h	d h	Stationary. d h	d h
Mars ...	Mar. 29 13	......	Dec. 24 9			
Jupiter...	Jan. 15 11	Aug. 5 5	May 6 22;	Nov. 1 14	June 6 2;	Oct. 4 1
Saturn...	Jan. 9 10	July 17 13	Apr. 17 23;	Oct. 15 17	May 7 22;	Sept. 25 20
Uranus...	Dec. 13 22	June 10 16	Mar. 12 5;	Sept. 11 11	Mar. 26 16;	Aug. 26 23
Neptune...	June [illegible]	Dec. [illegible]	Mar. [illegible];	Sept. [illegible]	Mar. [illegible];	Oct. [illegible]

1902.	Jupiter (♃).						Saturn (♄).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h n
Jan. 1	19 33 10	22 0 S.	29·8	20 50	0 52	4 54	19 16 53	22 6 S.	14·0	20 35	0 36	4 3
21	19 53 4	21 13	29·8	19 43	23 50	3 57	19 26 59	21 48	14·0	19 21	23 24	3 2
Feb. 10	20 12 34	20 18	30·2	18 38	22 51	3 4	19 36 39	21 28	14·2	18 10	22 15	2 2
Mar. 2	20 30 55	19 19	31·0	17 32	21 51	2 10	19 45 16	21 9	14·4	16 58	21 5	1 1
22	20 47 24	18 21	32·2	16 23	20 48	1 13	19 52 15	20 52	14·8	15 44	19 53	0
Apr. 11	21 1 16	17 28	33·8	15 13	19 43	0 13	19 57 5	20 40	15·2	14 29	18 39	22 4
May 1	21 11 45	16 46	35·8	14 1	18 35	23 9	19 59 22	20 35	15·8	13 14	17 23	21 3
21	21 18 4	16 23	38·2	12 47	17 23	21 59	19 58 57	20 38	16·2	11 53	16 3	20 1
June 10	21 19 36	16 21	40·6	11 27	16 5	20 43	19 55 59	20 48	16·8	10 33	14 42	18 5
30	21 16 8	16 41	43·0	10 8	14 43	19 18	19 50 59	21 3	17·0	9 10	13 18	17 2
July 20	21 8 22	17 20	44·4	8 46	13 17	17 48	19 44 55	21 21	17·0	7 48	11 54	17
Aug. 9	20 58 13	18 7	45·0	7 21	11 48	16 15	19 38 58	21 37	17·0	6 25	10 29	14 3
29	20 48 31	18 47	44·0	5 58	10 20	14 42	19 34 21	21 49	16·6	5 3	9 6	13
Sept. 18	20 41 59	19 12	42·2	4 36	8 55	13 14	19 31 56	21 56	16·2	3 43	7 45	11 4
Oct. 8	20 40 22	19 17	39·8	3 15	7 34	11 53	19 32 15	21 57	15·6	2 24	6 26	10 2
28	20 44 5	19 2	37·4	2 0	6 20	10 40	19 35 19	21 51	15·2	1 8	5 11	9 1
Nov. 17	20 52 38	18 27	35·2	0 45	5 9	9 33	19 40 54	21 39	14·8	23 56	3 58	8
Dec. 7	21 5 6	17 34	33·4	23 33	4 3	8 33	19 48 30	21 21	14·4	22 41	2 47	6 5
27	21 20 25	16 25 S.	32·0	22 24	3 0	7 36	19 57 31	20 58 S.	14·2	21 29	1 37	5 4

	Uranus (♅).						Neptune (♆).					
Jan. 1	17 9 50	23 0 S.	3·4	18 30	22 25	2 20	5 59 10	22 15 N.	2·7	3 9	11 16	19 2
31	17 16 33	23 8	3·5	16 40	20 34	0 28	5 59 59	22 16	2·7	1 8	9 15	17 2
Mar. 2	17 20 54	23 12	3·6	14 46	18 40	22 34	5 54 22	22 17	2·6	23 8	7 15	15 2
Apr. 1	17 21 59	23 14	3·6	12 49	16 43	20 37	5 54 52	22 18	2·6	21 10	5 18	13 2
May 1	17 19 49	23 12	3·7	10 49	14 43	18 37	5 57 25	22 20	2·6	19 15	3 23	11 3
31	17 15 16	23 8	3·7	8 47	12 41	16 35	6 1 28	22 21	2·5	17 21	1 29	9 3
June 30	17 10 3	23 2	3·7	6 42	10 37	14 32	6 6 14	22 21	2·5	15 24	23 32	7 4
July 30	17 6 2	22 57	3·6	4 40	8 36	12 32	6 10 47	22 20	2·5	13 30	21 38	5 4
Aug. 29	17 4 39	22 56	3·6	2 40	6 36	10 32	6 14 17	22 18	2·6	11 36	19 44	3 5
Sept. 28	17 6 32	22 58	3·5	0 44	4 40	8 36	6 15 59	22 17	2·6	9 39	17 47	1 5
Oct. 28	17 11 25	23 5	3·4	22 52	2 47	6 42	6 15 35	22 16	2·6	7 42	15 49	23 5
Nov. 27	17 18 26	23 12	3·3	21 2	0 56	4 50	6 13 13	22 16	2·7	5 42	13 49	21 5
Dec. 27	17 26 19	23 20 S.	3·3	19 9	23 2	2 56	6 9 43	22 17 N.	2·7	3 39	11 47	19 5

Ceres.				Pallas.			
1902.	R.A. Noon.	Dec. Noon.	Transits.	1902.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Sept. 30 ...	8 45	22 33 N.	20 10	Dec. 11 ...	6 39	32 10 S.	13 19
Oct. 12 ...	9 4	21 59	19 42	23 ...	6 29	32 51	12 23
24 ...	9 21	21 29	19 12	35 ...	6 19	32 12 S.	11 25
Nov. 5 ...	9 37	21 6	18 40				
17 ...	9 51	20 55 N.	18 7				

Juno.				Vesta.			
Feb. 14 ...	11 59	0 49 S.	14 22	June 26 ...	19 52	21 30 S.	13 34
26 ...	11 52	0 49 N.	13 27	July 8 ...	19 41	22 50	12 37
Mar. 10 ...	11 43	2 42	12 31	20 ...	19 29	24 9	11 38
22 ...	11 33	4 35	11 34	Aug. 1 ...	19 18	25 16	10 40
Apr. 3 ...	11 24	6 16 N.	10 39	13 ...	19 11	26 5 S.	9 45

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1902.

In the year 1902 there will be three Eclipses of the Sun and two of the Moon.

I.—*A Partial Eclipse of the Sun*, 1902, *April* 8.

Invisible at Greenwich. This Eclipse, which will be visible only from the northern part of N. America and the Arctic Regions, is unimportant. The magnitude of greatest Eclipse is 0·068 (Sun's diam. = 1).

II.—*A Total Eclipse of the Moon*, 1902, *April* 22.

Partly visible at Greenwich. The whole of the total phase will be seen from Eastern Europe, East and South Africa, Asia, and Australia. The Moon will rise totally eclipsed at Greenwich at 7^h 5^m.

	h	m		h	m
First contact with Penumbra ……	3	50·3	With shadow	5	0·2
Last ,, ,, ……	9	55·3	,, ,,	8	45·4
Beginning of Total Phase ………	6	10·2	End………	7	35·4

III.—*A Partial Eclipse of the Sun*, 1902, *May* 7.

Invisible at Greenwich. Visible only from New Zealand and the Pacific Ocean.

At Wellington (N. Z.) a Partial Eclipse will be visible. Magnitude 0·246 (Sun's diam. = 1). Begins May 7^d 20^h $19^{m}{\cdot}8$. Greatest Phase 7^d 21^h $4^{m}{\cdot}7$. Ends 7^d 21^h $52^{m}{\cdot}7$. Wellington Standard Mean Time (11½ hours fast on Greenwich).

IV.—*A Total Eclipse of the Moon*, 1902, *October* 16.

Partly visible at Greenwich.

	h	m		h	m
First contact with Penumbra	15	17·9	With shadow	16	16·9
Last ,, ,, ,,	20	48·9	,, ,,	19	49·9
Beginning of Total Phase …	17	18·6	End of Total Phase	18	48·2

The Moon will set totally eclipsed at Greenwich at 18^h 32^m.

V.—*A Partial Eclipse of the Sun*, *October* 30.

Greenwich Mean Time of Conjunction in R.A. 30^d 19^h 28^m 19^s.

	d	h	m		°	′		°	′	
Begins on the Earth generally Oct.	30	17	58·5	in Long.	19	46	E., Lat.	58	26	N.
Ends ,, ,, ,,	30	22	2·3	,,	106	8	E., Lat.	33	12	N.

	h	m	°	
At Greenwich a Partial Eclipse will be partly visible, ending	19	0·6	39	Angle from N. point.
At Oxford ,, ,, ,, ,,	19	0·4	39	
At Cambridge ,, ,, ,, ,,	19	4·7	42	

OCCULTATIONS, 1902. (Visible at Greenwich.)

Date.	Star.	Mag.	Disap.		Reap.	
			M. T.	P.	M. T.	P.
Jan.			h m	°	h m	°
1	50 Virginis ...	6·5	20 8	72	21 9	330
5	B.A.C. 5436 ...	6·0	17 9	126	18 14	260
12	c^1 Capricorni ...	5·2	4 17	73	5 27	238
13	κ Aquarii	5·5	6 23	10	7 1	301
18	D.M.+15° 430.	6·5	5 26	139	5 55	187
19	B.A.C. 1240 ...	5·7	3 24	60	4 22	274
19	D.M.+18° 594.	6·5	8 17	112	9 21	234
19	D.M.+18° 624.	5·9	13 2	75	13 58	284
20	B.A.C. 1647 ...	6·5	12 13	38	12 49	330
21	D.M.+18°1129	6·5	7 32	149	8 9	214
21	71 Orionis	5·1	8 15	22	8 39	343
22	68 Geminorum .	5·0	16 56	159	17 28	231
23	B.A.C. 2737 ...	6·3	6 3	38	6 31	339
24	κ Cancri	5·0	6 7	110	7 1	276
24	ω Leonis	5·6	17 23	61	18 0	342
26	p^5 Leonis	5·5	18 38	78	19 30	326
31	26 Libræ	6·5	18 17	68	19 26	320
Feb.						
1	β^2 Scorpii	5·2	19 10	123	20 29	255
1	β^1 Scorpii	3·0	19 10	124	20 28	254
12	ε Piscium	4·5	7 40	124	8 18	201
13	B.A.C. 609	6·2	8 29	63	9 28	270
14	σ Arietis	5·5	5 53	121	6 45	210
14	D M.+15° 430.	6·5	12 12	71	13 2†	276
15	B.A.C. 1240 ...	5·7	11 27	22	11 52	333
16	*i* Tauri	5·1	6 24	130	7 18	222
20	A^2 Cancri	5·8	6 13	116	7 16	272
20	60 Cancri	5·7	11 16	70	12 11	334
22	36 Sextantis ...	6·5	13 47	133	14 57	279
Mar.						
4	D.M. − 18° 5206	6·5	16 32	116	17 36	234
15	B.A.C. 1468 ...	6·5	10 51	57	11 36	306
17	26 Geminorum .	5·1	9 19	136	10 16	247
18	68 Geminorum .	5·0	5 48	170	6 14	211
20	ω Leonis	5·6	8 27	106	9 43	299
22	p^5 Leonis	5·5	11 19	148	12 24	265
25	86 Virginis......	6·0	15 13	85	16 24	312
28	ν Scorpii	4·5	13 41	119	15 0	265
31	D.M. − 19° 5134	6·5	13 24†	123	14 22	236
Apr.						
4	B.A.C. 7717 ...	6·5	15 54*	142	16 15	183
11	δ^3 Tauri	4·2	9 36	130	10 15	230
12	119 Tauri	4·6	11 23	86	12 11†	282
12	120 Tauri	5·3	11 51‡	92	12 38†	276
13	B.A.C. 2116 ...	6·5	11 17	92	12 8	287
14	68 Geminorum .	5·0	12 36	54	13 9	332
15	27 Cancri	5·6	11 47	167	12 17	229
21	α Virginis	1·2	11 46	148	12 52	257
25	B.A.C. 5580 ...	5·7	10 53	112	12 7	268
28	ρ^1 Sagittarii ...	3·9	16 20	80	17 44	254

Date.	Star.	Mag.	Disap.		R
			M. T.	P.	M. T
May			h m	°	h
1	c^1 Capricorni ...	5·2	13 12†	50	14
1	c^2 Capricorni ...	6·2	13 45†	114	14 3
11	51 Geminorum .	5·4	11 37†	171	11 5
12	12 Cancri	6·5	10 37	181	10 5
15	36 Sextantis ...	6·5	9 19	108	10 3
June					
2	54 Ceti	5·8	15 1	60	15 5
15	86 Virginis ...	6·0	10 15	110	11 3
18	ν Scorpii	4·5	9 37	118	10 5
21	D.M. − 19° 5134	6·5	9 7	92	10 2
25	B.A.C. 7717 ...	6·5	15 22	97	16 2
July					
2	δ^3 Tauri	4·2	14 3*	77	14 5
19	ρ^1 Sagittarii ...	3·9	10 58	30	11 5
22	c^2 Capricorni ...	6·2	8 54	356	9
24	12 Piscium	6·5	11 30	111	12 2
29	B.A.C. 1239 ...	6·5	12 35	35	13 1
29	D.M +17° 676.	6·5	14 14	28	14 5
29	B.A.C. 1272 ...	6·3	15 26	106	16 2
30	*m* Tauri	5·1	14 46	42	15 2
Aug.					
10	8 Libræ	5·3	8 29	132	9 3
10	α Libræ	3·0	8 41	142	9 3
18	c^1 Capricorni ...	5·2	17 1	0	17 2
Sept.					
15	B.A.C. 7717 ...	6·5	11 51	68	13
17	21 Piscium	6·1	11 31	359	12
21	B.A.C. 1119 ...	6·4	16 34	26	17 1
22	δ^1 Tauri	4·0	9 2	18	9 2
22	δ^2 Tauri	4·7	9 14	59	10
22	D.M.+17°750 (Mean)	6·5	13 47	0	13 5
23	115 Tauri	5·4	10 34	130	11 1
23	120 Tauri	5·3	13 28	17	13 5
24	B.A.C. 2116 ...	6·5	12 41	56	13 3
24	26 Geminorum .	5·1	17 32	68	18 3
25	68 Geminorum .	5·0	13 40	92	14 4
27	ω Leonis	5·6	15 22	177	15 3
Oct.					
5	D.M. − 17° 4431	6·5	5 19	60	6 2
12	ι^2 Capricorni ...	6·2	9 48	70	11
16	ζ^1 Piscium	4·2	10 17	100	11 1
19	D.M.+16° 569 .	6·4	11 46	68	12 5
19	δ^1 Tauri	4·0	17 8	84	18 1
19	δ^2 Tauri	4·7	17 43	108	18 4
21	D.M. +17°1182	6·5	11 12	89	12 1
21	D.M. +17°1214	6·5	14 34	120	15 3
22	λ Geminorum...	3·6	12 53	58	13 4
24	κ Cancri	5·0	11 24	99	12 1

† Star below horizon. * Star rising. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from t bottom of the Moon's inverted image towards the right.

te.	Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.				M.T.	P.	M.T.	
r.			h m	°	h m	°	Dec.			h m	°	h m	
	B.A.C. 7717 ...	6·5	5 33	47	6 49	266	13	δ^1 Tauri	4·0	13 53	86	14 56	2
	60 Cancri	5·7	12 6	94	13 7	291	13	δ^2 Tauri	4·7	14 27	110	15 23	24
	κ Cancri	5·0	18 42	103	19 51	299	14	120 Tauri	5·3	16 52	13	17 0	3
	ν Leonis	4·5	14 14	69	15 0	333	15	D.M.+12° 1182	6·5	5 47	94	6 38	2
							15	D.M.+17° 1214	6·5	8 36	134	9 19	2
							15	B.A.C. 2116 ...	6·5	13 37	101	14 46	2
	B.A.C. 6992 ...	6·7	7 32	34	8 24†	290	15	26 Geminorum .	5·1	18 24	29	18 42	3
	β Capricorni ...	3·4	7 38	38	8 33†	286	16	λ Geminorum .	3·6	5 49†	64	6 32	3
	12 Piscium......	6·5	5 41	60	6 59	247	16	68 Geminorum .	5·0	12 27	141	13 20	2
	ζ^1 Piscium	4·2	7 8	74	8 22	238	17	A^2 Cancri	5·8	18 37	96	19 36	3
	D.M.+16° 569.	6·4	8 43	75	9 52	263	20	B.A.C. 3873 ...	6·2	14 20	156	15 11	2

† Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
			h m	°				h m	°
Jan. 19	B.A.C. 1361 ...	6·5	15 17	0	Sept. 13	β Capricorni ...	3·4	6 48	347
Feb. 18	D.M.+17°1479	6·2	12 7	14	23	119 Tauri	4·6	13 5	354
20	A^1 Cancri	5·6	4 50	11	Oct. 12	c^1 Capricorni ...	5·2	9 55	336
20	κ Cancri.........	5·0	17 44	18	19	B.A.C. 1272 ...	6·3	10 16	347
Mar. 15	*i* Tauri	5·1	13 8	1	Nov. 11	21 Piscium ...	6·1	6 55	335
May 20	α Libræ	3·0	13 18	12	15	B.A.C. 1119 ...	6·4	9 1	345
Aug. 25	B.A.C. 1119 ...	6·4	10 11	348	Dec. 13	B.A.C. 1272 ...	6·3	7 14	347
Sept. 8	B.A.C. 5408 ...	6·5	9 50	1	14	119 Tauri	4·6	16 24	4
13	B.A.C. 6992 ...	6·7	6 39	347	17	A^1 Cancri	5·6	17 5	20

South Near Approaches.

			h m	°				h m	°
Jan. 12	c^2 Capricorni...	6·2	5 20	156	Oct. 20	115 Tauri	5·4	18 53	184
Feb. 16	*m* Tauri	5·1	14 21	183	22	51 Geminorum.	5·4	11 17	183
June 20	B.A.C. 6081 ...	6·5	14 39	172	Nov. 18	D M.+16°1363	6·5	15 20	189
July 26	ζ^1 Piscium	4·2	13 21	157	Dec. 14	115 Tauri	5·4	14 16	182
Oct. 14	12 Piscium......	6·5	12 1	156	22	B.A.C. 4394 ...	5·9	18 34	205

JUPITER'S SATELLITES, 1902.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

February.

d	h	m	
12	2	15	i. T.
	20	37	i. *e.*
	23	24	i. O.
13	8	6	ii. *t.*
	11	2	ii. T.
	18	26	i. *t.*
	20	46	i. T.
14	5	29	iii. *e.*
	11	4	ii. O.
	15	6	i. *e.*
	17	54	O.
15	2	18	ii. *e.*
	6	15	ii. O.
	12	56	i. *t.*
	15	16	i. T.
16	9	35	i. *e.*
	12	25	i. O.
	21	31	ii. *t.*
17	0	27	ii. T.
	7	27	i. *t.*
	9	46	i. T.
	21	41	iii. *t.*
18	1	15	iii. T.
	3	40	iv. *t.*
	4	3	i. *e.*
	6	55	i. O.
	8	1	iv. T.
	15	36	ii. *e.*
	19	41	ii. O.
19	1	57	i. *t.*
	4	16	i. T.
	22	31	i. *e.*
20	1	25	i. O.
	10	56	ii. *t.*
	13	51	ii. T.
	20	27	i. *t.*
	22	47	i. T.
21	9	29	iii. *e.*
	15	33	iii. O.
	17	0	i. *e.*
	19	56	i. O.
22	4	53	ii. *e.*
	9	4	ii. O.
	14	57	i. *t.*
	17	17	i. T.
23	11	28	i. *e.*
	14	26	i. O.
24	0	20	ii. *t.*
	3	16	ii. T.
	9	27	i. *t.*
	11	47	i. T.
25	2	7	iii. *t.*
	5	43	iii. T.

Feb. (con.).

d	h	m	
25	5	57	i. *e.*
	8	56	i. O.
	18	11	ii. *e.*
	22	28	ii. O.
26	3	57	i. *t.*
	6	17	i. T.
	7	5	iv. *e.*
	11	14	iv. E.
	13	37	iv. *o.*
	18	3	iv. O.
27	0	25	i. *e.*
	3	26	i. O.
	13	45	ii. *t.*
	16	40	ii. T.
	22	28	i. *t.*
28	0	47	i. T.
	13	28	iii. *e.*
	18	54	i. *e.*
	20	0	iii. O.
	21	56	i. O.

March.

d	h	m	
1	7	28	ii. *e.*
	11	52	ii. O.
	16	58	i. *t.*
	19	17	i. T.
2	13	22	i. *e.*
	16	26	i. O.
3	3	9	iii. *t.*
	6	5	ii. T.
	11	28	i. *t.*
	13	48	i. T.
4	6	33	iii. *t.*
	7	51	i. *e.*
	10	10	iii. T.
	10	56	i. O.
	20	46	ii. *e.*
5	1	16	ii. O.
	5	58	i. *t.*
	8	17	i. T.
6	2	19	i. *e.*
	5	26	i. O.
	16	33	ii. *t.*
	19	29	ii. T.
7	0	8	iv. *t.*
	0	28	i. *t.*
	2	47	i. T.
	4	38	iv. T.
	17	28	iii. *e.*
	20	48	i. *e.*
	23	56	i. O.
8	0	26	iii. O.

Mar. (con.).

d	h	m	
8	10	3	ii. *e.*
	14	40	ii. O.
	18	58	i. *t.*
	21	17	i. T.
9	15	16	i. *e.*
	18	26	i. O.
10	5	57	ii. *t.*
	8	53	ii. T.
	13	27	i. *t.*
	15	47	i. T.
11	9	45	i. *e.*
	10	57	iii. *t.*
	12	56	i. O.
	14	35	iii. T.
	23	21	ii. *e.*
12	4	3	ii. O.
	7	57	i. *t.*
	10	17	i. T.
13	4	13	i. *e.*
	7	26	i. O.
	19	20	ii. *t.*
	22	16	ii. T.
14	2	27	i. *t.*
	4	47	i. T.
	21	26	iii. *e.*
	22	42	i. *e.*
15	0	52	iii. E.
	1	8	iv. *e.*
	1	11	iii. *o.*
	1	56	i. O.
	4	49	iii. O.
	5	22	iv. E.
	9	53	iv. *o.*
	12	38	ii. *e.*
	14	27	iv. O.
	17	26	ii. O.
	20	57	i. *t.*
	23	17	i. T.
16	17	10	i. *e.*
	20	26	i. O.
17	8	44	ii. *t.*
	11	41	ii. T.
	15	27	i. *t.*
	17	47	i. T.
18	11	39	i. *e.*
	14	56	i. O.
	15	19	iii. *t.*
	18	58	iii. T.
19	1	56	ii. *e.*
	6	49	ii. O.
	9	57	i. *t.*
	12	17	i. T.
20	6	7	i. *e.*

Mar. (con.).

d	h	m	
20	9	26	i. O.
	22	7	ii. *t.*
21	1	4	ii. T.
	4	26	i. *t.*
	6	46	i. T.
22	0	36	i. *e.*
	1	25	iii. *e.*
	3	56	i. O.
	4	51	iii. E.
	5	32	iii. *o.*
	9	11	iii. O.
	15	13	ii. *e.*
	20	11	ii. O.
	22	56	i. *t.*
23	1	16	i. T.
	19	4	i. *e.*
	20	14	iv. *t.*
	22	26	i. O.
24	0	51	iv. T.
	11	31	ii. *t.*
	14	27	ii. T.
	17	26	i. *t.*
	19	46	i. T.
25	13	33	i. *e.*
	16	55	i. O.
	19	39	iii. *t.*
	23	18	iii. T.
26	4	30	ii. *e.*
	9	33	ii. O.
	11	55	i. *t.*
	14	15	i. T.
27	8	1	i. *e.*
	11	25	i. O.
28	0	53	ii. *t.*
	3	50	ii. T.
	6	25	i. *t.*
	8	45	i. T.
29	2	30	i. *e.*
	5	25	iii. *e.*
	5	55	i. O.
	8	51	iii. E.
	9	51	iii. *o.*
	13	30	iii. C.
	17	47	ii. *e.*
	22	55	ii. O.
30	0	54	i. *t.*
	3	14	i. T.
	20	58	i. *e.*
31	0	24	i. O.
	14	16	ii. *t.*
	17	12	ii. T.
	19	11	iv. *e.*
	19	24	i. *t.*

Mar. (con.).

d	h	m	
31	21	44	i. T.
	23	30	iv. E.

April.

d	h	m	
1	5	44	iv. *o.*
	10	26	iv. O.
	15	27	i. *e.*
	18	54	i. O.
	23	56	iii. *t.*
2	3	36	iii. T.
	7	5	ii. *e.*
	12	16	ii. O.
	13	53	i. *t.*
	16	13	i. T.
3	9	55	i. *e.*
	13	23	i. O.
4	3	38	ii. *t.*
	6	34	ii. T.
	8	22	i. *t.*
	10	43	i. T.
5	4	24	i. *e.*
	7	53	i. O.
	9	24	iii. *e.*
	12	51	iii. E.
	14	7	iii. *o.*
	17	48	iii. O.
	20	22	ii. *e.*
6	1	37	ii. O.
	2	52	i. *t.*
	5	12	i. T.
	22	52	i. *e.*
7	2	22	i. O.
	17	0	ii. *t.*
	19	57	ii. T.
	21	21	i. *t.*
	23	41	i. T.
8	17	20	i. *e.*
	20	52	i. O.
9	4	10	iii. *t.*
	7	51	iii. T.
	9	39	ii. *e.*
	14	58	ii. O.
	15	50	i. *t.*
	15	51	iv. *t.*
	18	10	i. T.
	20	35	iv. T.
10	11	49	i. *e.*
	15	21	i. O.
11	6	21	ii. *t.*
	9	18	ii. T.
	10	19	i. *t.*
	12	40	i. T.

Apr. (

d	h	m
12	6	17
	9	5
	13	24
	16	51
	18	21
	22	2
	22	56
13	4	18
	4	49
	7	9
14	0	46
	4	19
	19	43
	22	39
	23	17
15	1	38
	19	14
	22	49
16	8	22
	12	3
	12	13
	17	38
	17	47
	20	7
17	13	14
	13	43
	17	18
	17	37
18	1	2
	5	49
	9	3
	12	0
	12	16
	14	36
19	8	11
	11	47
	17	23
	20	51
	22	31
20	1	30
	2	13
	6	45
	6	57
	9	5
21	2	39
	6	16
	22	24
22	1	13
	1	21
	3	33
	21	7
23	0	45
	12	3

Apr. (con.).

d	h	m	
3	14	47	ii. *e.*
	16	12	iii. T.
	19	42	i. *t.*
	20	16	ii. O.
	22	2	i. T.
4	15	35	i. *e.*
	19	14	i. O.
5	11	44	ii. *t.*
	14	11	i. *t.*
	14	40	ii. T.
	16	31	i. T.
6	10	3	i. *e.*
	10	51	iv. *t.*
	13	44	i. O.
	15	40	iv. T.
	21	22	iii. *e.*
27	0	51	iii. E.
	2	38	iii. *o.*
	4	4	ii. *e.*
	6	20	iii. O.
	8	40	i. *t.*
	9	35	ii. O.
	11	0	i. T.
28	4	34	i. *e.*
	8	12	i. O.
29	1	4	ii. *t.*
	3	9	i. *t.*
	4	1	ii. T.
	5	29	i. T.
	23	2	i. *e.*
30	2	40	i. O.
	16	35	iii. *t.*
	17	21	ii. *e.*
	20	17	iii. T.
	21	37	i. *t.*
	22	53	ii. O.
	23	57	i. T.

May.

d	h	m	
1	17	31	i. *e.*
	21	9	i. O.
2	14	23	ii. *t.*
	16	6	i. *t.*
	17	19	ii. T.
	18	26	i. T.
3	11	59	i. *e.*
	15	38	i. O.
4	1	21	iii. *e.*
	4	50	iii. E.
	6	38	ii. *e.*
	6	41	iii. *o.*
	7	18	iv. *e.*
	10	23	iii. O.
	10	34	i. *t.*
	11	44	iv. E.
	12	10	ii. O.
	12	54	i. T.
	19	40	iv. *o.*
5	0	30	iv. O.
	6	27	i. *e.*
	10	7	i. O.
6	3	42	ii. *t.*
	5	3	i. *t.*

May (oon.).

d	h	m	
6	6	39	ii. T.
	7	23	i. T.
7	0	56	i. *e.*
	4	35	i. O.
	19	55	ii. *e.*
	20	36	iii. *t.*
	23	31	i. *t.*
8	0	19	iii. T.
	1	27	ii. O.
	1	51	i. T.
	19	24	i. *e.*
	23	4	i. O.
9	17	0	ii. *t.*
	17	59	i. *t.*
	19	56	ii. T.
	20	20	i. T.
10	13	53	i. *e.*
	17	33	i. O.
11	5	20	iii. *e.*
	8	49	iii. E.
	9	11	ii. *e.*
	10	40	iii. *o.*
	12	27	i. *t.*
	14	23	iii. O.
	14	44	ii. O.
	14	48	i. T.
12	8	21	i. *e.*
	12	0	i. O.
13	5	6	iv. *t.*
	6	18	ii. *t.*
	6	56	i. *t.*
	9	15	ii. T.
	9	16	i. T.
	9	58	iv. T.
14	2	50	i. *e.*
	6	29	i. O.
	22	28	ii. *e.*
15	0	33	iii. *t.*
	1	24	i. *t.*
	3	44	i. T.
	4	0	ii. O.
	4	16	iii. T.
	21	18	i. *e.*
16	0	57	i. O.
	19	35	ii. *t.*
	19	52	i. *t.*
	22	12	i. T.
	22	32	ii. T.
17	15	47	i. *e.*
	19	25	i. O.
18	9	19	iii. *e.*
	11	45	ii. *e.*
	12	49	iii. E.
	14	20	i. *t.*
	14	36	iii. *o.*
	16	40	i. T.
	17	16	ii. O.
	18	18	iii. O.
19	10	15	i. *e.*
	13	53	i. O.
20	8	48	i. *t.*
	8	53	ii. *t.*
	11	8	i. T.
	11	49	ii. T.

May (con.).

d	h	m	
21	1	21	iv. *e.*
	4	44	i. *e.*
	5	51	iv. E.
	8	21	i. O.
	13	27	iv. *o.*
	18	20	iv. O.
22	1	2	ii. *e.*
	3	15	i. *t.*
	4	26	iii. *t.*
	5	36	i. T.
	6	31	ii. O.
	8	8	iii. T
	23	12	i. *e.*
23	2	49	i. O.
	21	43	i. *t.*
	22	9	ii. *t.*
24	0	4	i. T.
	1	5	ii. T.
	17	41	i. *e.*
	21	17	i. O.
25	13	19	iii. *e.*
	14	19	ii. *e.*
	16	11	i. *t.*
	16	49	iii. E.
	18	27	iii. *o.*
	18	31	i. T.
	19	45	ii. O.
	22	10	iii. O.
26	12	9	i. *e.*
	15	45	i. O.
27	10	38	i. *t.*
	11	25	ii. *t.*
	12	59	i. T.
	14	21	ii. T.
28	6	38	i. *e.*
	10	13	i. O.
29	3	36	ii. *e.*
	5	6	i. *t.*
	7	27	i. T.
	8	14	iii. *t.*
	8	59	ii. O.
	11	57	iii. T.
	22	29	iv. *t.*
30	1	6	i. *e.*
	3	23	iv. T.
	4	40	i. O.
	23	34	i. *t.*
31	0	40	ii. *t.*
	1	54	i. T.
	3	36	ii. T.
	19	35	i. *e.*
	23	8	i. O.

June.

d	h	m	
1	16	53	ii. *e.*
	17	19	iii. *e.*
	18	1	i. *t.*
	20	21	i. T.
	20	49	iii. E.
	22	13	ii. O.
	22	14	iii. *o.*
2	1	56	iii. O.
	14	3	i. *e.*

June (con.).

d	h	m	
2	17	36	i. O.
3	12	29	i. *t.*
	13	55	ii. *t.*
	14	50	i. T.
	16	51	ii. T.
4	8	32	i. *e.*
	12	3	i. O.
5	6	10	ii. *e.*
	6	56	i. *t.*
	9	16	i. T.
	11	26	ii. O.
	11	58	iii. *t.*
	15	40	iii. T.
6	3	0	i. *e.*
	6	31	i. O.
	19	25	iv. *e.*
	23	58	iv. E.
7	1	23	i. *t.*
	3	9	ii. *t.*
	3	43	i. T.
	6	5	ii. T.
	6	19	iv. *o* .
	11	14	iv. O.
	21	29	i. *e.*
8	0	58	i. O.
	19	27	ii. *e.*
	19	50	i. *t.*
	21	18	iii. *e.*
	22	11	i. T.
9	0	38	ii. O.
	0	49	iii. E.
	1	56	iii. *o.*
	5	39	iii O.
	15	57	i. *e.*
	19	25	i. O.
10	14	17	i. *t.*
	16	23	ii. *t.*
	16	38	i. T.
	19	19	ii. T.
11	10	26	i. *e.*
	13	52	i. O.
12	8	44	ii. *e.*
	8	44	i. *t.*
	11	5	i. T.
	13	50	ii. O.
	15	37	iii. *t.*
	19	20	iii. T.
13	4	54	i. *e.*
	8	20	i. O.
14	3	11	i. *t.*
	5	32	i. T.
	5	36	ii. *t.*
	8	32	ii. T.
	23	23	i. *e.*
15	2	47	i. O.
	14	55	iv. *t.*
	19	50	iv. T.
	21	38	i. *t.*
	22	1	ii. *e.*
	23	59	i. T.
16	1	17	iii. *e.*
	3	1	ii. O.
	4	49	iii. E.
	5	33	iii. *o.*

June (con.).

d	h	m	
16	9	16	iii. O.
	17	52	i. *e.*
	21	14	i. O.
17	16	5	i. *t.*
	18	25	i. T.
	18	49	ii. *t.*
	21	45	ii. T.
18	12	20	i. *e.*
	15	41	i. O.
19	10	32	i. *t.*
	11	18	ii. *e.*
	12	52	i. T.
	16	12	ii. O.
	19	12	iii. *t.*
	22	55	iii. T.
20	6	49	i. *e.*
	10	8	i. O.
21	4	58	i. *t.*
	7	19	i. T.
	8	0	ii. *t.*
	10	56	ii. T.
22	1	17	i. *e.*
	4	34	i. O.
	23	25	i. *t.*
23	0	35	ii. *e.*
	1	45	i. T.
	5	17	iii. *e.*
	5	22	ii. O.
	8	48	iii. E.
	9	5	iii. *o.*
	12	48	iii. O.
	13	30	iv. *e.*
	18	6	iv. E.
	19	46	i. *e.*
	22	15	iv. *o.*
	23	1	i. O.
24	3	9	iv. O.
	17	52	i. *t.*
	20	12	i. T.
	21	12	ii. *t.*
25	0	8	ii. T.
	14	14	i. *e.*
	17	28	i. O.
26	12	18	i. *t.*
	13	52	ii. *e.*
	14	39	i. T.
	18	32	ii. O.
	22	43	iii. *t.*
27	2	26	iii. T.
	8	43	i. *e.*
	11	55	i. O.
28	6	45	i. *t.*
	9	5	i. T.
	10	23	ii. *t.*
	13	19	ii. T.
29	3	11	i. *e.*
	6	21	i. O.
30	1	11	i. *t.*
	3	9	ii. *e.*
	3	32	i. T.
	7	41	ii. O.
	9	16	iii. *e.*
	16	16	iii. O.
	21	40	i. *e.*

July.

d	h	m	
1	0	48	i. O
	19	38	i. *t.*
	21	58	i. T.
	23	34	ii. *t.*
2	2	30	ii. T
	6	27	iv. *t*
	11	22	iv.
	16	9	i. *e.*
	19	14	i. O
3	14	4	i. *t.*
	16	24	i. T.
	16	26	ii. *e.*
	20	50	ii. O
4	2	9	iii. *t*
	5	52	iii. T
	10	37	i. *e.*
	13	41	i. O.
5	8	30	i. *t.*
	10	51	i. T.
	12	43	ii. *t.*
	15	39	ii. T
6	5	6	i. *e.*
	8	7	i. O.
7	2	57	i. *t.*
	5	17	i. T.
	5	43	ii. *e.*
	9	59	ii. O
	13	16	iii. *e*
	19	40	iii.
	23	34	i. *e.*
8	2	34	i. O.
	21	23	i. *t.*
	23	43	i. T.
9	1	54	ii. *t.*
	4	50	ii. T
	18	3	i. *e.*
	21	0	i. O.
10	7	36	iv. *e*
	12	14	iv. E
	13	18	iv. *o*
	15	49	i. *t.*
	18	9	i. T.
	18	13	iv. O
	19	0	ii. *e.*
	23	7	ii. O
11	5	32	iii. *t*
	9	15	iii. T
	12	32	i. *e.*
	15	26	i. O.
12	10	15	i. *t.*
	12	35	i. T.
	15	2	ii. *t.*
	17	58	ii. T
13	7	0	i. *e.*
	9	52	i. O.
14	4	41	i. *t.*
	7	2	i. T.
	8	18	ii. *e.*
	12	15	ii. O
	17	16	iii. *e.*
	23	3	iii. O
15	1	29	i. *e.*
	4	19	i. O.
	23	7	i. *t.*

July (con.).

d	h	m	
16	1	28	i. T.
	4	12	ii. *t.*
	7	8	ii. T.
	19	57	i. *e.*
	22	45	i. O.
17	17	33	i. *t.*
	19	54	i. T.
	21	35	ii. *e.*
18	1	22	ii. O.
	8	51	iii. *t.*
	12	34	iii. T.
	14	26	i. *e.*
	17	11	i. O.
	21	14	iv. *t.*
19	2	9	iv. T.
	11	59	i. *t.*
	14	20	i. T.
	17	20	ii. *t.*
	20	16	ii. T.
20	8	55	i. *e.*
	11	37	i. O.
21	6	25	i. *t.*
	8	45	i. T.
	10	52	ii. *e.*
	14	30	ii. O.
	21	17	iii. *e.*
22	2	22	iii. O.
	3	23	i. *e.*
	6	3	i. O.
23	0	51	i. *t.*
	3	11	i. T.
	6	28	ii. *t.*
	9	24	ii. T.
	21	52	i. *e.*
24	0	29	i. O.
	19	17	i. *t.*
	21	37	i. T.
25	0	10	ii. *e.*
	3	37	ii. O.
	12	9	iii. *t.*
	15	51	iii. T.
	16	21	i. *e.*
	18	55	i. O.
26	13	43	i. *t.*
	16	3	i. T.
	19	36	ii. *t.*
	22	32	ii. T.
27	1	43	iv. *e.*
	8	38	iv. O.
	10	49	i. *e.*
	13	21	i. O.
28	8	9	i. *t.*
	10	29	i. T.
	13	27	ii. *e.*
	16	44	ii. O.
29	1	17	iii. *e.*
	5	18	i. *e.*
	5	39	iii. O.
	7	47	i. O.
30	2	35	i. *t.*
	4	55	i. T.
	8	44	ii. *t.*
	11	40	ii. T.
	23	47	i. *e.*

July (con.).

d	h	m	
31	2	13	i. O.
	21	0	i. *t.*
	23	21	i. T.

August.

d	h	m	
1	2	45	ii. *e.*
	5	50	ii. O.
	15	25	iii. *t.*
	18	15	i. *e.*
	19	8	iii. T.
	20	39	i. O.
2	15	26	i. *t.*
	17	47	i. T.
	21	52	ii. *t.*
3	0	48	ii. T.
	12	44	i. *e.*
	15	5	i. O.
4	9	52	i. *t.*
	11	34	iv. *t.*
	12	13	i. T.
	16	1	ii. *o.*
	16	29	iv. T.
	18	57	ii. O.
5	5	12	iii. *o.*
	7	10	i. *o.*
	8	55	iii. O.
	9	31	i. O.
6	4	18	i. *t.*
	6	38	i. T.
	11	0	ii. *t.*
	13	56	ii. T.
7	1	36	i. *o.*
	3	57	i. E.
	22	44	i. *t.*
8	1	4	i. T.
	5	8	ii. *o.*
	8	11	ii. E.
	18	41	iii. *t.*
	20	2	i. *o.*
	22	24	iii. T.
	22	26	i. E.
9	17	10	i. *t.*
	19	30	i. T.
10	0	6	ii. *t.*
	3	2	ii. T.
	14	28	i. *o.*
	16	55	i. E.
11	11	36	i. *t.*
	13	56	i. T.
	18	15	ii. *o.*
	21	28	ii. E.
12	8	28	iii. *o.*
	8	54	i. *o.*
	11	24	i. E.
	12	51	iii. E.
	17	54	iv. *o.*
13	0	32	iv. E.
	6	2	i. *t.*
	8	22	i. T.
	13	15	ii. *t.*
	16	11	ii. T.
14	3	20	i. *o.*
	5	52	i. E.

Aug. (con.).

d	h	m	
15	0	28	i. *t.*
	2	48	i. T.
	7	22	ii. *o.*
	10	46	ii. E.
	21	46	i. *o.*
	21	59	iii. *t.*
16	0	21	i. E.
	1	41	iii. T.
	18	54	i. *t.*
	21	14	i. T.
17	2	23	ii. *t.*
	5	19	ii. T.
	16	13	i. *o.*
	18	50	i. E.
18	13	20	i. *t.*
	15	40	i. T.
	20	29	ii. *o.*
19	0	5	ii. E.
	10	39	i. *o.*
	11	45	iii. *o.*
	13	18	i. E.
	16	51	iii. E.
20	7	46	i. *t.*
	10	6	i. T.
	15	31	ii. *t.*
	18	27	ii. T.
21	1	54	iv. *t.*
	5	5	i. *o.*
	6	49	iv. T.
	7	47	i. E.
22	2	12	i. *t.*
	4	32	i. T.
	9	37	ii. *o.*
	13	22	ii. E.
	23	31	i. *o.*
23	1	17	iii. *t.*
	2	16	i. E.
	4	59	iii. T.
	20	38	i. *t.*
	22	58	i. T.
24	4	39	ii. *t.*
	7	35	ii. T.
	17	57	i. *o.*
	20	45	i. E.
25	15	4	i. *t.*
	17	25	i. T.
	22	45	ii. *o.*
26	2	40	ii. E.
	12	24	i. *o.*
	15	4	iii. *o.*
	15	13	i. E.
	20	52	iii. E.
27	9	30	i. *t.*
	11	51	i. T.
	17	48	ii. *t.*
	20	44	ii. T.
28	6	50	i. *o.*
	9	42	i. E.
29	3	57	i. *t.*
	6	17	i. T.
	8	19	iv. *o.*
	11	53	ii. *o.*
	13	14	iv. O.
	14	0	iv. *e.*

Aug. (con.).

d	h	m	
29	15	58	ii. E.
	18	42	iv. E.
30	1	16	i. *o.*
	4	11	i. E.
	4	37	iii. *t.*
	8	20	iii. T.
	22	23	i. *t.*
31	0	44	i. T.
	6	56	ii. *t.*
	9	53	ii. T.
	19	43	i. *o.*
	22	40	i. E.

September.

d	h	m	
1	16	50	i. *t.*
	19	10	i. T.
2	1	2	ii. *o.*
	5	16	ii. E.
	14	9	i. *o.*
	17	8	i. E.
	18	27	iii. *o.*
3	0	53	iii. E.
	11	16	i. *t.*
	13	36	i. T.
	20	6	ii. *t.*
	23	2	ii. T.
4		6	i. *o.*
	11	37	i. E.
5	5	42	i. *t.*
	8	3	i. T.
	14	11	ii. *o.*
	18	35	ii. E.
6	3	2	i. *o.*
	6	6	i. E.
	8	1	iii. *t.*
	11	43	iii. T.
	16	41	iv. *t.*
	21	34	iv. T.
7	0	9	i. *t.*
	2	30	i. T.
	9	15	ii. *t.*
	12	11	ii. T.
	21	29	i. *o.*
8	0	35	i. E.
	18	37	i. *t.*
	20	57	i. T.
9	3	21	ii. *o.*
	7	53	ii. E.
	15	55	i. *o.*
	19	4	i. E.
	21	53	iii. *o.*
10	4	55	iii. E.
	13	2	i. *t.*
	15	23	i. T.
	22	26	ii. *t.*
11	1	22	ii. T.
	10	22	i. *o.*
	13	32	i. E.
12	7	29	i. *t.*
	9	50	i. T.
	16	31	ii. *o.*
	21	11	ii. E.
13	4	49	i. *o.*

Sept. (con.).

d	h	m	
13	8	1	i. E.
	11	28	iii. *t.*
	15	11	iii. T.
14	1	56	i. *t.*
	4	17	i. T.
	11	36	ii. *t.*
	14	32	ii. T.
	23	16	i. *o.*
	23	21	iv. *o.*
15	2	30	i. E.
	4	14	iv. O.
	8	9	iv. *e.*
	12	52	iv. E.
	20	23	i. *t.*
	22	44	i. T.
16	5	41	ii. *o.*
	10	30	ii. E.
	17	43	i. *o.*
	20	59	i. E.
17	1	23	iii. *o.*
	5	5	iii. O.
	5	23	iii. *e.*
	8	56	iii. E.
	14	50	i. *t.*
	17	11	i. T.
18	0	47	ii. *t.*
	3	43	ii. T.
	12	10	i. *o.*
	15	28	i. E.
19	9	17	i. *t.*
	11	38	i. T.
	18	53	ii. *o.*
	23	48	ii. E.
20	6	37	i. *o.*
	9	56	i. E.
	15	1	iii. *t.*
	18	43	iii. T.
21	3	45	i. *t.*
	6	5	i. T.
	13	58	ii. *t.*
	16	55	ii. T.
22	1	5	i. *o.*
	4	25	i. E.
	22	12	i. *t.*
23	0	32	i. T.
	8	5	ii. *o.*
	8	13	iv. *t.*
	13	6	iv. T.
	13	7	ii. E.
	19	32	i. *o.*
	22	54	i. E.
24	4	57	iii. *o.*
	8	39	iii. O.
	9	24	iii. *e.*
	12	57	iii. E.
	16	39	i. *t.*
	19	0	i. T.
25	3	10	ii. *t.*
	6	7	ii. T.
	13	59	i. *o.*
	17	23	i. E.
26	11	7	i. *t.*
	13	27	i. T.
	21	17	ii. *o.*

Sept. (con.

d	h	m	
27	2	25	ii.
	8	27	i.
	11	52	i.
	18	38	iii.
	22	20	iii.
28	5	34	i.
	7	55	i.
	16	23	ii.
	19	19	ii.
29	2	54	i.
	6	21	i.
30	0	2	i.
	2	22	i.
	10	30	ii.
	15	44	ii.
	21	22	i.

October.

d	h	m	
1	0	49	i.
	8	36	iii
	12	18	iii
	13	24	iii
	15	18	iv.
	16	58	iii
	18	29	i.
	20	10	iv.
	20	50	i.
2	2	20	iv.
	5	36	ii.
	7	4	iv.
	8	33	ii.
	15	49	i.
	19	18	i.
3	12	57	i.
	15	17	i.
	23	44	ii.
4	5	3	ii.
	10	17	i.
	13	47	i.
	22	20	iii.
5	2	3	iii.
	7	25	i.
	9	45	i.
	18	50	ii.
	21	46	ii.
6	4	45	i.
	8	16	i.
7	1	53	i.
	4	13	i.
7	12	59	ii.
	18	22	ii.
	23	13	i.
8	2	45	i.
	12	20	iii.
	16	2	iii.
	17	26	iii.
	20	21	i.
	20	59	iii.
	22	41	i.
9	8	4	ii.
	11	1	ii.
	17	41	i.
	21	14	i.

Oct. (con.).

d	h	m	
10	0	41	iv. *t.*
	5	34	iv. T.
	14	49	i. *t.*
	17	9	i. T.
11	2	14	ii. *o.*
	7	40	ii. E.
	12	9	i. *o.*
	15	42	i. E.
12	2	7	iii. *t.*
	5	49	iii. T.
	9	17	i. *t.*
	11	37	i. T.
	21	19	ii. *t.*
13	0	15	ii. T.
	6	37	i. *o.*
	10	11	i. E.
14	3	45	i. *t.*
	6	5	i. T.
	15	29	ii. *o.*
	20	59	ii. E.
15	1	5	i. *o.*
	4	40	i. E.
	16	8	iii. *o.*
	19	51	iii. O.
	21	27	iii. *e.*
	22	13	i. *t.*
16	0	34	i. T.
	1	0	iii. E.
	10	34	ii. *t.*
	13	31	ii. T.
	19	33	i. *o.*
	23	9	i. E.
17	16	42	i. *t.*
	19	2	i. T.
18	4	46	ii. *o.*
	8	14	iv. *o.*
	10	18	ii. E.
	13	6	iv. O.
	14	2	i. *o.*
	17	38	i. E.
	20	32	iv. *e.*
19	1	15	iv. E.
	5	58	iii. *t.*
	9	40	iii. T.
	11	10	i. *t.*
	13	31	i. T.
	23	50	ii. *t.*
20	2	47	ii. T.
	8	30	i. *o.*
	12	7	i. E.
21	5	38	i. *t.*
	7	59	i. T.
	18	3	ii. *o.*
	23	37	ii. E.
22	2	58	i. *o.*
	6	35	i. E.
	20	2	iii. *o.*
	23	45	iii. O.
23	0	7	i. *t.*
	1	29	iii. *e.*
	2	27	i. T.
	5	2	iii. E.
	13	7	ii. *t.*
	16	3	ii. T.

Oct. (con.).

d	h	m	
23	21	27	i. *o.*
24	1	4	i. E.
	18	36	i. *t.*
	20	56	i. T.
25	7	20	ii. *o.*
	12	56	ii. E.
	15	55	i. *o.*
	19	33	i. E.
26	9	54	iii. *t.*
	13	4	i. *t.*
	13	36	iii. T.
	15	25	i. T.
	18	9	iv. *t.*
	23	2	iv. T.
27	2	24	ii. *t.*
	5	20	ii. T.
	10	24	i. *o.*
	14	2	i. E.
28	7	33	i. *t.*
	9	53	i. T.
	20	38	ii. *o.*
29	2	15	ii. E.
	4	53	i. *o.*
	8	31	i. E.
30	0	1	iii. *o.*
	2	2	i. *t.*
	3	43	iii. O.
	4	22	i. T.
	5	31	iii. *e.*
	9	4	iii. E.
	15	41	ii. *t.*
	18	38	ii. T.
	23	21	i. *o.*
31	3	0	i. E.
	20	31	i. *t.*
	22	51	i. T.

November.

d	h	m	
1	9	57	ii. *o.*
	15	34	ii E.
	17	50	i. *o.*
	21	28	i. E.
2	13	54	iii. *t.*
	15	0	i. *t.*
	17	20	i. T.
	17	37	iii. T.
3	4	59	ii. *t.*
	7	56	ii. T.
	12	19	i. *o.*
	15	57	i. E.
4	2	8	iv. *o.*
	7	1	iv. O.
	9	29	i. *t.*
	11	49	i. T.
	14	43	iv. *e.*
	19	27	iv. E.
	23	17	ii. *o.*
5	4	53	ii. E.
	6	48	i. *o.*
	10	26	i. E.
6	3	58	i. *t.*
	4	3	iii. *o.*
	6	18	i. T.

Nov. (con.).

d	h	m	
6	7	46	iii. O.
	9	32	iii. *e.*
	13	5	iii. E.
	18	18	ii. *t.*
	21	14	ii. T.
7	1	17	i. *o.*
	4	55	i. E.
	22	27	i. *t.*
8	0	47	i. T.
	12	37	ii. *o.*
	18	12	ii. E.
	19	46	i. *o.*
	23	24	i. E.
9	16	56	i. *t.*
	17	59	iii. *t.*
	19	17	i. T.
	21	42	iii. T.
10	7	37	ii. *t.*
	10	33	ii. T.
	14	15	i. *o.*
	17	53	i. E.
11	11	25	i. *t.*
	13	46	i. T.
12	1	57	ii. *o.*
	7	32	ii. E.
	8	44	i. *o.*
	12	21	i. E.
	12	32	iv. *t.*
	17	26	iv. T.
13	5	55	i. *t.*
	8	10	iii. *o.*
	8	15	i. T.
	11	52	iii. O.
	13	33	iii. *e.*
	17	6	iii. E.
	20	57	ii. *t.*
	23	53	ii. T.
14	3	14	i. *o.*
	6	50	i. E.
15	0	24	i. *t.*
	2	44	i. T.
	15	18	ii. *o.*
	20	50	ii. E.
	21	43	i. *o.*
16	1	19	i. E.
	18	54	i. *t.*
	21	14	i. T.
	22	8	iii. *t.*
17	1	50	iii. T.
	10	17	ii. *t.*
	13	13	ii. T.
	16	12	i. *o.*
	19	48	i. E.
18	13	23	i. *t.*
	15	43	i. T.
19	4	39	ii. *o.*
	10	10	ii. E.
	10	42	i. *o.*
	14	17	i. E.
20	7	53	i. *t.*
	10	13	i. T.
	12	19	iii. *o.*
	16	2	iii. O.
	17	35	iii. *e.*

Nov. (con.).

d	h	m	
20	20	56	iv. *o.*
	21	7	iii. E.
	23	37	ii. *t.*
21	1	50	iv. O.
	2	33	ii. T.
	5	11	i. *o.*
	8	46	i. E.
	8	56	iv. *e.*
	13	39	iv. E.
22	2	22	i. *t.*
	4	43	i. T.
	18	1	ii. *o.*
	23	29	ii. E.
	23	41	i. *o.*
23	3	14	i. E.
	20	52	i. *t.*
	23	13	i. T.
24	2	21	iii. *t.*
	6	4	iii. T.
	12	58	ii. *t.*
	15	54	ii. T.
	18	10	i. *o.*
	21	43	i. E.
25	15	22	i. *t.*
	17	42	i. T.
26	7	23	ii. *o.*
	12	40	i. *o.*
	12	48	ii. E.
	16	12	i. E.
27	9	52	i. *t.*
	12	12	i. T.
	16	33	iii. *o.*
	20	16	iii. O.
	21	36	iii. *e.*
28	1	8	iii. E.
	2	19	ii. *t.*
	5	15	ii. T.
	7	10	i. *o.*
	10	41	i. E.
29	4	22	i. *t.*
	6	42	i. T.
	7	43	iv. *t.*
	12	37	iv. T.
	20	46	ii. *o.*
30	1	39	i. *o.*
	2	7	ii. E.
	5	9	i. E.
	22	52	i. *t.*

December.

d	h	m	
1	1	12	i. T.
	6	37	iii. *t.*
	10	19	iii. T.
	15	40	ii. *t.*
	18	37	ii. T.
	20	9	i. *o.*
	23	38	i. E.
2	17	21	i. *t.*
	19	42	i. T.
3	10	9	ii. *o.*
	14	39	i. *o.*
	15	27	ii. E.
	18	7	i. E.

Dec. (con.).

d	h	m	
4	11	51	i. *t.*
	14	12	i. T.
	20	50	iii. *o.*
5	0	33	iii. O.
	1	37	iii. *e.*
	5	2	ii. *t.*
	5	9	iii. E.
	7	58	ii. T.
	9	9	i. *o.*
	12	36	i. E.
6	6	21	i. *t.*
	8	42	i. T.
	23	33	ii. *o.*
7	3	39	i. *o.*
	4	45	ii. E.
	7	5	i. E.
	16	26	iv. *o.*
	21	22	iv. O.
8	0	52	i. *t.*
	3	12	i. T.
	3	8	iv. *e.*
	7	50	iv. E.
	10	55	iii. *t.*
	14	38	iii. T.
	18	24	ii. *t.*
	21	20	ii. T.
	22	9	i. *o.*
9	1	33	i. E.
	19	21	i. *t.*
	21	42	i. T.
10	12	56	ii. *o.*
	16	38	i. *o.*
	18	5	ii. E.
	20	2	i. E.
11	13	52	i. *t.*
	16	12	i. T.
12	1	10	iii. *o.*
	4	52	iii. O
	5	39	iii. *e.*
	7	47	ii. *t.*
	9	11	iii. E.
	10	43	ii. T.
	11	8	i. *o.*
	14	31	i. E.
13	8	22	i. *t.*
	10	42	i. T.
14	2	21	ii. *o.*
	5	38	i. *o.*
	7	24	ii. E.
	9	0	i. E.
15	2	52	i. *t.*
	5	12	i. T.
	15	16	iii. *t.*
	18	59	iii. T.
	21	10	ii. *t.*
16	0	6	ii. T.
	0	8	i. *o.*
	3	29	i. E.
	3	34	iv. *t.*
	8	29	iv. T.
	21	22	i. *t.*
	23	42	i. T.
17	15	45	ii. o.
	18	39	i. o.

Dec. (con.)

d	h	m	
17	20	44	ii.
	21	57	i.
18	15	53	i. *t*
	18	14	i.
19	5	32	iii.
	9	15	iii.
	9	41	iii.
	10	33	ii.
	13	9	i.
	13	12	iii.
	13	29	ii.
	16	26	i.
20	10	23	i. *t*
	12	43	i.
21	5	10	ii.
	7	39	i. *o*
	10	2	ii.
	10	55	i.
22	4	53	i. *t*
	7	13	i.
	19	40	iii.
	23	23	iii.
	23	56	ii.
23	2	9	i. *o*
	2	52	ii.
	5	24	i.
	23	23	i. *t.*
24	1	44	i.
	12	31	iv.
	17	27	iv.
	18	35	ii.
	20	39	i. *o.*
	21	21	iv.
	23	22	ii.
	23	52	i. E
25	2	2	iv.
	17	54	i. *t.*
	20	14	i. T
26	9	56	iii.
	13	20	ii.
	13	39	iii.
	13	42	iii.
	15	9	i. *o.*
	16	16	ii.
	17	13	iii.
	18	21	i. E
27	12	24	i. *t.*
	14	44	i. T
28	8	0	ii. *o*
	9	40	i. *o.*
	12	41	ii.
	12	50	i. E
29	6	54	i. *t.*
	9	15	i. T
30	0	6	iii.
	2	44	ii. *t.*
	3	49	iii.
	4	10	i. *o.*
	5	40	ii. T
	7	19	i. E.
31	1	25	i. *t.*
	3	45	i. T
	21	26	ii.
	22	40	i.

The satellites of Jupiter will be invisible from the beginning of the year until February 12, Jupiter being too near the Sun.

Satellites I. & II. *disappear* at eclipse on the *p* side from February to July, and *reappear* on the *f* side from August to December.

Satellite III. *disappears* and *reappears* on the *p* side from February to July, on the *f* side from August to December.

Satellite IV. *disappears* and *reappears* on the *p* side from February to July, and on the *f* side from August to December.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Feb. $18\frac{1}{2}^h$	March. $17\frac{1}{2}^h$	April. 16^h	May. 15^h	June. 14^h	July. $12\frac{1}{2}^h$	Aug. 11^h	Sept. $9\frac{1}{2}^h$	Oct. 8^h	Nov. $6\frac{1}{2}^h$	Dec. $5\frac{1}{2}^h$	Day.
1		♃○234	2○43●	431○2	432○1	○1243	1○324	32○14	43○12	412○3	41○32	1
2		○1234	♃3○24	♃43○1	41○32	214○3	3○124	31○24	♃1○43	4○312	432○1	2
3		21○34	3○124	423○●	♃♃4○3	42○13	321○4	○1324	2○134	♃431○	4312○	3
4		32○14	321○4	14○23	42○13	43○2●	♃32○4	2○34●	1○234	32○1●	43○12	4
5		31○24	2○14●	○1423	♃41○2	431○2	4○312	21○34	♃3○24	31○24	♃41○3	5
6		♃3○41	1○234	21○34	34○12	432○1	♃41○3	♃○412	32○4●	1○24●	42○13	6
7		241○●	○2134	2○314	321○4	413○2	42○13	♃341○	321○4	2○134	4○23●	7
8		4○123	21○34	31○24	32○14	4○123	41○23	432○1	3○124	12○34	1○32●	8
9		4○23●	♃♃3○2	3○214	1○324	421○3	43○12	431○2	1○243	○1324	32○14	9
10		421○3	34○12	32○4●	○1234	24○13	4321○	4○132	24○13	13○24	321○4	10
11		423○1	4321○	1○324	2○134	31○24	432○1	421○3	41○23	32○14	3○124	11
12	31○24	431○2	42○1●	○1423	1○234	♃3○24	4○2●●	♃42○3	43○12	31○4●	1○324	12
13	♃32○4	43○21	41○23	124○3	3○124	32○14	1○243	4○312	432○●	♃43○2	2○134	13
14	2○134	2413○	4○213	42○13	312○4	13○24	2○134	341○2	4321○	42○3●	1○34●	14
15	1○234	○2413	421○3	431○2	32○41	○1324	1○234	32○1●	43○12	421○3	1○324	15
16	○2134	○243●	43○1●	43○21	41○32	12○34	3○124	31○4●	41○23	4○123	♃32○1	16
17	21○34	♃2○34	3○2●●	4321○	4○123	2○134	321○4	○3124	42○13	413○2	3421○	17
18	34○1●	♃2○14	321○4	♃4○●●	42○3●	♃1○42	32○14	21○34	14○3●	432○1	43○12	18
19	341○2	31○24	23○14	4○123	41○3●	♃34○2	3○24●	♃2○34	♃○142	431○●	413○2	19
20	432○1	3○214	1○234	412○3	43○12	432○1	1○243	○324●	321○4	43○12	42○13	20
21	42○3●	231○4	○2134	2○13●	4312○	431○●	24○13	31○24	321○4	2○43●	41○3●	21
22	41○23	○134●	21○34	13○24	432○1	4○132	41○3●	32○14	3○124	21○43	♃4○32	22
23	4○123	1○423	♃○14●	3○124	1○32●	412○3	43○12	♃31○●	1○234	○1234	432○1	23
24	421○3	♃42○3	3○24●	321○4	○1243	42○13	4312○	4○12●	2○134	13○24	3214○	24
25	43○1●	42○13	♃32○4	○14●●	21○34	♃41○2	432○1	412○3	1○34●	32○14	3○142	25
26	31○42	431○2	234○1	○234●	♃○34●	34○12	431○2	42○13	○3142	312○4	13○24	26
27	32○14	43○12	41○23	12○34	3○124	32○4●	♃4○23	4○32●	3421○	3○124	2○134	27
28	21○4●	4321○	4○123	2○134	312○4	312○4	42○13	431○2	♃342○	21○34	12○34	28
29		42○31	421○3	13○42	32○14	○3124	12○3●	432○1	43○2●	♃2○43	○1234	29
30		41○23	42○31	34○12	1○24●	12○34	3○124	4312○	41○2●	4○123	♃3○4●	30
31		42○13		4321○		2○134	312○4		42○13		321○4	31

The circle (○) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

EPHEMERIS OF JUPITER'S SATELLITE V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of 11h·96 to these.

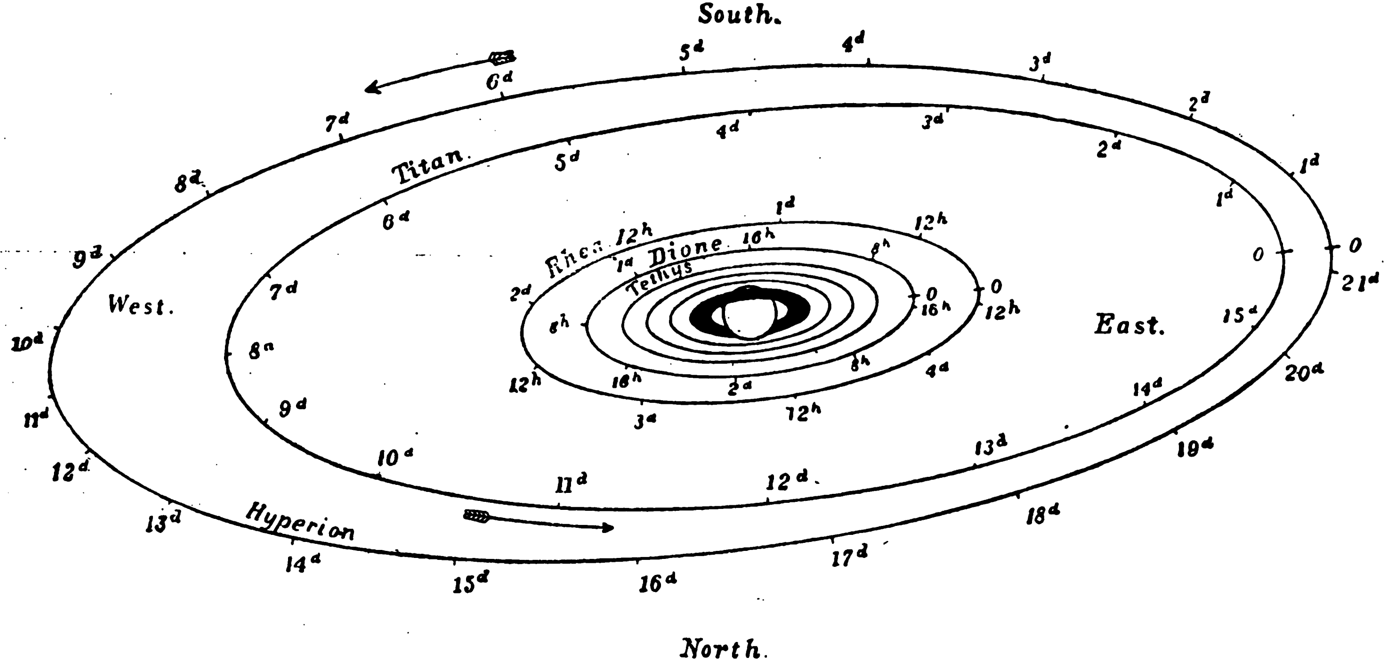
	d	h		d	h		d	h		d	h
May	16	8·69	July	5	4·21	Aug.	24	23·54	Oct.	13	19·17
	26	7·81		15	3·29	Sept.	3	22·64		23	18·33
June	5	6·92		25	2·37		13	21·75	Nov.	2	17·50
	15	6·03	Aug.	4	1·45		23	20·88		12	16·67
	25	5·12		14	0·54	Oct.	3	20·02		22	15·84

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, „	16 29 „	0·1844	67 8
1902, inferred	16 17 „	0·1848	67 5

The horizontal force is given in C. G. S. measure.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN AT OPPOSITION, 1902, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
2	16·4	1	20·3	1	1·5	1	3·8	1	7·4	1	9·7	1	12·0	1	15·8	1	18·4
3	15·0	2	18·9	2	0·1	2	2·4	2	6·1	2	8·3	2	10·6	2	14·4	2	17·0
4	13·7	3	17·5	2	22·7	3	1·0	3	4·7	3	6·9	3	9·2	3	13·1	3	15·6
5	12·3	4	16·2	3	21·3	3	23·6	4	3·3	4	5·5	4	7·9	4	11·7	4	14·2
6	10·9	5	14·8	4	20·0	4	22·2	5	1·9	5	4·1	5	6·5	5	10·3	5	12·9
7	9·5	6	13·4	5	18·6	5	20·8	6	0·5	6	2·8	6	5·1	6	8·9	6	11·5
8	8·2	7	12·0	6	17·2	6	19·5	6	23·1	7	1·4	7	3·7	7	7·5	7	10·1
9	6·8	8	10·6	7	15·8	7	18·1	7	21·7	8	0·0	8	2·3	8	6·2	8	8·7
10	5·4	9	9·3	8	14·4	8	16·7	8	20·3	8	22·6	9	0·9	9	4·8	9	7·4
11	4·0	10	7·9	9	13·0	9	15·3	9	18·9	9	21·2	9	23·6	10	3·4	10	6·0
12	2·6	11	6·5	10	11·7	10	13·9	10	17·6	10	19·8	10	22·2	11	2·0	11	4·6
13	1·3	12	5·1	11	10·3	11	12·5	11	16·2	11	18·4	11	20·8	12	0·6	12	3·3
13	23·9	13	3·7	12	8·9	12	11·2	12	14·8	12	17·1	12	19·4	12	23·3	13	1·9
14	22·5	14	2·4	13	7·5	13	9·8	13	13·4	13	15·7	13	18·0	13	21·9	14	0·5
15	21·1	15	1·0	14	6·1	14	8·4	14	12·0	14	14·3	14	16·7	14	20·5	14	23·1
16	19·8	15	23·6	15	4·7	15	7·0	15	10·6	15	12·9	15	15·3	15	19·1	15	21·8
17	18·4	16	22·2	16	3·3	16	5·6	16	9·2	16	11·5	16	13·9	16	17·8	16	20·4
18	17·0	17	20·8	17	2·0	17	4·3	17	7·9	17	10·1	17	12·5	17	16·4	17	19·0
19	15·6	18	19·5	18	0·6	18	2·9	18	6·5	18	8·8	18	11·1	18	15·0	18	17·6
20	14·2	19	18·1	18	23·2	19	1·5	19	5·1	19	7·4	9	9·8	19	13·6	19	16·2
21	12·9	20	16·7	19	21·8	20	0·1	20	3·7	20	6·0	20	8·4	20	12·3	20	14·9
22	11·5	21	15·3	20	20·4	20	22·7	21	2·3	21	4·6	21	7·0	21	10·9	21	13·5
23	10·1	22	13·9	21	19·0	21	21·3	22	0·9	22	3·2	22	5·6	22	9·5	22	12·1
24	8·7	23	12·5	22	17·6	22	19·9	22	23·5	23	1·8	23	4·2	23	8·1	23	10·7
25	7·3	24	11·2	23	16·3	23	18·5	23	22·1	24	0·5	24	2·9	24	6·8	24	9·4
26	6·0	25	9·8	24	14·9	24	17·1	24	20·7	24	23·1	25	1·5	25	5·4	25	8·0
27	4·6	26	8·4	25	13·5	25	15·7	25	19·4	25	21·7	26	0·1	26	4·0		
28	3·2	27	7·0	26	12·1	26	14·3	26	18·0	26	20·3	26	22·7	27	2·6		
29	1·8	28	5·6	27	10·7	27	13·0	27	16·6	27	18·9	27	21·3	28	1·2		
30	0·4	29	4·2	28	9·3	28	11·6	28	15·2	28	17·5	28	20·0	28	23·9		
30	23·1	30	2·8	29	7·9	29	10·2	29	13·8	29	16·2	29	18·6	29	22·5		
31	21·7			30	6·6	30	8·8	30	12·4	30	14·8	30	17·2	30	21·1		
				31	5·2			31	11·1	31	13·4			31	19·7		

ENCELADUS.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
3	19·3	1	14·1	1	17·6	2	5·9	1	0·3	1	12·4	2	0·6	2	4·1	1	7·7
5	4·2	2	23·0	3	2·5	3	14·8	2	9·2	2	21·3	3	9·5	3	13·0	2	16·6
6	13·1	4	7·9	4	11·3	4	23·7	3	18·1	4	6·2	4	18·4	4	21·9	4	1·5
7	21·0	5	16·7	5	20·2	6	8·5	5	2·9	5	15·0	6	3·2	6	6·8	5	10·4
9	6·9	7	1·6	7	5·1	7	17·4	6	11·8	6	23·9	7	12·1	7	15·7	6	19·3
10	15·8	8	10·5	8	14·0	9	2·3	7	20·6	8	8·8	8	21·0	9	0·6	8	4·2
12	0·7	9	19·4	9	22·9	10	11·2	9	5·5	9	17·7	10	5·9	10	9·4	9	13·1
13	9·6	11	4·3	11	7·8	11	20·0	10	14·4	11	2·5	11	14·8	11	18·3	10	22·0
14	18·5	12	13·2	12	16·7	13	4·9	11	23·2	12	11·4	12	23·7	13	3·2	12	6·9
16	3·4	13	22·1	14	1·6	14	13·7	13	8·1	13	20·3	14	8·6	14	12·1	13	15·8
17	12·3	15	7·0	15	10·5	15	22·6	14	17·0	15	5·2	15	17·4	15	21·0	15	0·7
18	21·2	16	15·9	16	19·3	17	7·5	16	1·8	16	14·0	17	2·3	17	5·9	16	9·6
20	6·1	18	0·7	18	4·2	18	16·4	17	10·7	17	22·9	18	11·2	18	14·8	17	18·5
21	15·0	19	9·6	19	13·1	20	1·2	18	19·6	19	7·8	19	20·1	19	23·7	19	3·4
22	23·9	20	18·5	20	22·0	21	10·1	20	4·5	20	16·7	21	5·0	21	8·6	20	12·3
24	8·7	22	3·4	22	6·8	22	19·0	21	13·4	22	1·5	22	13·9	22	17·5	21	21·2
25	17·6	23	12·3	23	15·7	24	3·9	22	22·2	23	10·4	23	22·8	24	2·4	23	6·1
27	2·5	24	21·2	25	0·6	25	12·7	24	7·1	24	19·3	25	7·6	25	11·3	24	15·0
28	11·4	26	6·0	26	9·5	26	21·6	25	16·0	26	4·2	26	16·5	26	20·2		
29	20·3	27	14·9	27	18·4	28	6·5	27	0·9	27	13·1	28	1·4	28	5·1		
31	5·2	28	23·8	29	3·3	29	15·4	28	9·8	28	21·9	29	10·3	29	14·0		
		30	8·7	30	12·1			29	18·6	30	6·8	30	19·2	30	22·8		
				31	21·0			31	3·5	31	15·7						

East Elongations.

TETHYS.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
4 1·8	1 9·6	1 14·6	2 16·7	1 0·0	2 2·0	1 6·7	1 11·6	2 14·1
5 23·1	3 6·9	3 11·9	4 14·0	2 21·3	3 23·3	3 4·0	3 8·9	4 11·4
7 20·4	5 4·2	5 9·2	6 11·3	4 18·6	5 20·5	5 1·3	5 6·2	6 8·7
9 17·7	7 1·6	7 6·5	8 8·6	6 15·9	7 17·8	6 22·6	7 3·5	8 6·0
11 15·1	8 22·9	9 3·8	10 5·8	8 13·2	9 15·1	8 19·9	9 0·8	10 3·4
13 12·4	10 20·2	11 1·1	12 3·1	10 10·5	11 12·4	10 17·2	10 22·1	12 0·7
15 9·7	12 17·5	12 22·4	14 0·4	12 7·8	13 9·7	12 14·5	12 19·5	13 22·0
17 7·0	14 14·8	14 19·7	15 21·7	14 5·0	15 7·0	14 11·8	14 16·8	15 19·4
19 4·4	16 12·1	16 17·0	17 19·0	16 2·3	17 4·3	16 9·1	16 14·1	17 16·7
21 1·7	18 9·4	18 14·3	19 16·3	17 23·6	19 1·6	18 6·4	18 11·4	19 14·0
22 23·0	20 6·7	20 11·6	21 13·6	19 20·9	20 22·9	20 3·7	20 8·8	21 11·3
24 20·3	22 4·0	22 8·9	23 10·9	21 18·2	22 20·2	22 1·0	22 6·1	
26 17·6	24 1·4	24 6·2	25 8·2	23 15·5	24 17·5	23 22·3	24 3·4	
28 15·0	25 22·7	26 3·5	27 5·5	25 12·8	26 14·8	25 19·6	26 0·7	
30 12·3	27 20·0	28 0·8	29 2·8	27 10·1	28 12·1	27 17·0	27 22·1	
	29 17·3	29 22·1		29 7·4	30 9·4	29 14·3	29 19·4	
				31 4·7			31 16·7	

DIONE.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
6 6·1	2 15·2	2 17·9	1 20·3	1 22·5	1 0·6	2 20·5	2 23·0	2 1·9
8 23·8	5 9·0	5 11·6	4 14·0	4 16·1	3 18·3	[illegible] 14·2	5 16·7	4 19·6
11 17·5	8 2·7	8 5·3	7 7·6	7 9·8	6 11·9	8 7·8	8 10·5	7 13·3
14 11·2	10 20·4	10 22·9	10 1·3	10 3·4	9 5·6	11 1·5	11 4·2	10 7·1
17 4·9	13 14·1	13 16·6	12 18·9	12 21·1	11 23·2	13 19·2	13 21·9	13 0·8
19 22·7	16 7·8	16 10·3	15 12·6	15 14·7	14 16·9	16 12·9	16 15·6	15 18·5
22 16·4	19 1·5	19 3·9	18 6·2	18 8·4	17 10·5	19 6·6	19 9·3	18 12·3
25 10·1	21 19·2	21 21·6	20 23·9	21 2·0	20 4·2	22 0·3	22 3·0	21 6·0
28 3·8	24 12·9	24 15·3	23 17·5	23 19·7	22 21·9	24 17·9	24 20·7	23 23·7
30 21·5	27 6·5	27 9·0	26 11·2	26 13·3	25 15·5	27 11·6	27 14·4	
	30 0·2	30 2·6	29 4·8	29 7·0	28 9·2	30 5·3	30 8·1	
					31 2·8			

RHEA.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
5 5·0	1 8·1	2 23·3	3 14·0	5 4·3	1 6·2	1 20·6	3 11·5	4 2·9
9 17·5	5 20·6	7 11·7	8 2·4	9 16·6	5 18·5	6 9·0	8 0·0	8 15·4
14 6·1	10 9·1	12 0·1	12 14·7	14 4·9	10 6·9	10 21·4	12 12·4	13 4·0
18 18·6	14 21·6	16 12·5	17 3·1	18 17·3	14 19·2	15 9·8	17 0·9	17 16·5
23 7·1	19 10·0	21 0·9	21 15·4	23 5·6	19 7·6	19 22·2	21 13·4	22 5·1
27 19·6	23 22·5	25 13·3	26 3·7	27 17·9	23 19·9	24 10·6	26 1·9	26 17·6
	28 10·9	30 1·7	30 16·0		28 8·3	28 23·0	30 14·4	

TITAN.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
13 10·5	14 10·0	16 7·9	1 6·0	3 1·4	3 20·2	4 15·7	6 13·0	7 11·9
29 10·5	30 9·2		17 3·9	18 22·8	19 17·8	20 14·0	22 12·2	23 12·0

East Elongations.

HYPERION.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
10 20·6	1 7·7 22 18·2	14 3·1	4 10·3 25 16·6	16 21·4	7 1·7 28 6·0	18 10·3	9 15·4 30 20·9	21 3·4

IAPETUS.

	d h	d h	d h	d h
E. Elongation	May 14 4·6,	July 31 9·8,	Oct. 18 7·2,	
Inf. Conjunction ...	Mar. 14 17·8,	June 2 10·8,	Aug. 19 13·2,	Nov. 7 0·7
W. Elongation	Apr. 4 15·6,	June 22 19·2,	Sept. 8 22·8,	Nov. 27 23·8
Sup. Conjunction ...	Apr. 25 2·9,	July 12 16·8,	Sept. 29 2·9,	

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 20	7 18·5	35·04	13·98	23 30·8 N.	23 44·7 N.
Feb. 9	7 21·2	35·38	13·76	22 53·8 N.	23 36·6 N.
Mar. 1	7 22·9	36·04	13·68	22 19·0 N.	23 28·3 N.
21	7 23·8	36·97	13·75	21 49·6 N.	23 19·8 N.
April 10	7 24·2	38·14	13·96	21 28·7 N.	23 11·1 N.
30	7 24·3	39·42	14·34	21 19·1 N.	23 2·4 N.
May 20	7 24·4	40·73	14·84	21 22·0 N.	22 53·4 N.
June 9	7 24·4	41·84	15·41	21 36·8 N.	22 44·3 N.
29	7 24·1	42·59	15·96	22 0·7 N.	22 35·1 N.
July 19	7 23·5	42·83	16·38	22 28·8 N.	22 25·6 N.
Aug. 8	7 22·5	42·52	16·57	22 55·7 N.	22 16·1 N.
28	7 21·5	41·71	16·48	23 16·6 N.	22 6·4 N.
Sept. 17	7 21·0	40·57	16·15	23 28·0 N.	21 56·5 N.
Oct. 7	7 21·1	39·27	15·64	23 28·7 N.	21 46·5 N.
27	7 21·9	37·99	15·03	23 18·1 N.	21 36·3 N.
Nov. 16	7 23·1	36·87	14·38	22 57·0 N.	21 26·0 N.
Dec. 6	7 24·3	35·99	13·74	22 26·1 N.	21 15·6 N.
26	7 25·0	35·41	13·14	21 47·5 N.	21 5·0 N.

SATELLITES OF URANUS.

East Elongations.

ARIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
11	3·7	1	7·9	3	3·5	3	10·6	2	17·6	1	23·8	2	5·1
13	16·2	3	20·5	5	16·1	5	23·2	5	6·2	4	12·3	4	17·5
16	4·7	6	9·0	8	4·7	8	11·8	7	18·7	7	0·8	7	5·9
18	17·3	8	21·6	10	17·2	11	0·4	10	7·2	9	13·2	9	18·4
21	5·8	11	10·2	13	5·8	13	13·0	12	19·8	12	1·7		
23	18·3	13	22·7	15	18·4	16	1·5	15	8·3	14	14·1		
26	6·9	16	11·3	18	7·0	18	14·1	17	20·8	17	2·5		
28	19·4	18	23·9	20	19·6	21	2·7	20	9·3	19	14·9		
		21	12·5	23	8·2	23	15·3	22	21·8	22	3·4		
		24	1·1	25	20·8	26	3·9	25	10·3	24	15·8		
		26	13·7	28	9·4	28	16·5	27	22·8	27	4·2		
		29	2·3	30	22·0	31	5·1	30	11·3	29	16·7		
		31	14·9										

UMBRIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
10	4·4	5	1·9	3	3·7	2	6·0	4	11·6	2	12·3	1	11·6
14	7·9	9	5·5	7	7·5	6	9·7	8	15·2	6	15·7	5	14·8
18	11·5	13	9·2	11	11·2	10	13·5	12	18·8	10	19·1	9	18·0
22	15·0	17	12·9	15	15·0	14	17·2	16	22·4	14	22·4	13	21·2
26	18·6	21	16·6	19	18·7	18	20·9	21	1·9	19	1·7	18	0·3
30	22·3	25	20·3	23	22·5	23	0·6	25	5·4	23	5·0	22	3·4
		30	0·0	28	2·2	27	4·3	29	8·8	27	8·3	26	6·5
						31	8·0						

TITANIA.

March.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
30	5·5	4	3·1	8	3·2	4	9·9	8	9·5	3	12·9	8	5·9
April.		12	20·9	16	21·4	13	4·0	17	2·9	12	5·6	16	21·6
7	22·7	21	14·9	25	15·7	21	22·1	25	20·1	20	21·9	25	13·1
16	15·9	30	9·0			30	15·9			29	14·0		
25	9·4												

OBERON.

April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h
20	2·4	3	15·1	13	8·1	10	12·2	6	14·7	2	14·0
		17	4·3	26	22·2	24	1·8	20	2·8	16	0·3
		30	18·0							29	9·6

Apparent Apsides.

		Apparent Distances.			
	Position Angle.	Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
April 14	241·5	14·2	19·8	32·5	43·5
June 13	253·5	14·6	20·3	33·3	44·6
August 12	266·5	14·2	19·7	32·4	43·3

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		April.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
4	11·8	2	21·4	4	6·8	2	16·1	1	17·9	6	0·2	5	9·7
10	8·9	8	18·5	10	3·9			7	14·9	11	21·3	11	6·8
16	6·0	14	15·6	16	0·9	September.		13	12·0	17	18·4	17	3·9
22	3·2	20	12·7	21	22·0	14	2·9	19	9·0	23	15·5	23	1·1
28	0·3	26	9·8	27	19·0	19	23·9	25	6·1	29	12·6	28	22·2
						25	20·9	31	3·1				

Position-angle and distance of the apse:—Feb. 5, 259°·4, 16″·7; Sept. 29, 265°·3, 16″·4; Dec. 18, 263°·8, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1902.	P.	D.	L.	1902.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 2 8	−3 12	118 55	July 5	− 1 1	+3 27	197 8
6	− 0 19	3 46	53 5	10	+ 1 16	3 59	130 58
11	2 44	4 17	347 15	15	3 32	4 28	64 50
16	5 7	4 48	281 24	20	5 45	4 55	358 40
21	7 26	5 15	215 35	25	7 54	5 20	292 30
26	9 40	5 40	149 45	30	9 59	5 44	226 22
31	11 48	6 3	83 54	Aug. 4	11 59	6 5	160 14
Feb. 5	13 50	6 23	18 4	9	13 54	6 24	94 8
10	15 44	6 40	312 14	14	15 42	6 39	28 3
15	17 30	6 53	246 25	19	17 23	6 53	321 58
20	19 8	7 4	180 34	24	18 57	7 3	255 53
25	20 36	7 11	114 43	29	20 23	7 10	189 49
Mar. 2	21 56	7 14	48 51	Sept. 3	21 41	7 14	123 46
7	23 5	7 15	342 59	8	22 50	7 15	57 44
12	24 5	7 12	277 6	13	23 52	7 13	351 44
17	24 55	7 5	211 11	18	24 43	7 7	285 43
22	25 35	6 56	145 16	23	25 24	6 59	219 42
27	26 4	6 43	79 19	28	25 56	6 48	153 43
Apr. 1	26 22	6 28	13 22	Oct. 3	26 18	6 33	87 45
6	26 29	6 10	307 23	8	26 29	6 15	21 47
11	26 27	5 49	241 24	13	26 29	5 55	315 50
16	26 12	5 25	175 23	18	26 17	5 32	249 52
21	25 47	4 59	109 20	23	25 55	5 6	183 56
26	25 10	4 32	43 16	28	25 20	4 38	118 0
May 1	24 23	4 2	337 12	Nov. 2	24 34	4 8	52 4
6	23 24	3 30	271 7	7	23 25	3 36	346 8
11	22 15	2 57	204 59	12	22 25	3 2	280 13
16	20 56	2 24	138 51	17	21 4	2 28	214 18
21	19 27	1 49	72 43	22	19 31	1 50	148 24
26	17 49	1 13	6 34	27	17 48	1 13	82 30
31	16 2	0 37	300 24	Dec. 2	15 55	+0 36	16 36
June 5	14 8	−0 1	234 14	7	13 53	−0 4	310 43
10	12 6	+0 35	168 3	12	11 43	0 42	244 50
15	10 0	1 11	101 53	17	9 26	1 19	178 58
20	7 49	1 46	35 41	22	7 6	1 58	113 6
25	5 34	2 21	329 31	27	4 42	2 34	47 14
30	− 3 18	+2 55	263 20	32	+ 2 15	−3 10	341 22

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 24′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1902·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris ...	0 3 40	−39 46·4	58.	S Orionis	5 24 10	− 4 46·2
2.	S Sculptoris ...	0 10 25	−32 35·4	59.	S Camelopardi .	5 30 26	+68 44·7
3.	T Andromedæ .	0 17 16	+26 27·1	60.	U Aurigæ	5 35 46	+31 59·5
4.	T Cassiopeiæ ...	0 17 55	+55 15·0	61.	S Columbæ	5 43 15	−31 43·6
5.	S Toucani	0 18 29	−62 13·2	62.	R Columbæ......	5 46 45	−29 13·1
6.	R Andromedæ .	0 18 51	+38 2·1	63.	U Orionis	5 50 0	+20 9·5
7.	S Ceti	0 19 4	− 9 52·3	64.	R Octantis	5 56 11	−86 26·1
8.	T Sculptoris ...	0 24 23	−38 27·0	65.	η Geminorum...	6 8 58	+22 32·2
9.	Y Cephei	0 31 27	+79 49·1	66.	V Aurigæ	6 16 27	+47 42·7
10.	U Cassiopeiæ ...	0 40 52	+47 42·3	67.	V Monocerotis..	6 17 47	− 2 8·8
11.	V Andromedæ .	0 44 46	+35 7·2	68.	T Monocerotis...	6 19 56	+ 7 8·4
12.	X Sculptoris ...	0 44 48	−35 27·1	69.	W Geminorum .	6 29 21	+15 24·4
13.	W Cassiopeiæ...	0 49 6	+58 2·0	70.	S Lyncis	6 36 7	+58 0·4
14.	U Cephei	0 53 34	+81 20·9	71.	X Geminorum..	6 40 51	+30 22·5
15.	U Sculptoris ...	1 6 55	−30 38·1	72.	W Monocerotis .	6 47 36	− 7 1·7
16.	U Andromedæ .	1 9 54	+40 12·0	73.	R Lyncis.........	6 53 13	+55 28·2
17.	S Cassiopeiæ ...	1 12 26	+72 5·7	74.	ζ Geminorum...	6 58 18	+20 42·8
18.	S Piscium	1 12 27	+ 8 24·9	75.	R Geminorum .	7 1 27	+22 51·3
19.	U Piscium	1 17 47	+12 21·2	76.	V Canis Min. ...	7 1 40	+ 9 1·3
20.	R Sculptoris ...	1 22 28	−33 3·0	77.	R Canis Min. ...	7 3 19	+10 10·7
21.	R Piscium	1 25 35	+ 2 22·6	78.	L^2 Puppis	7 10 33	−44 29·0
22.	X Cassiopeiæ ...	1 49 54	+58 46·7	79.	R Canis Maj. ...	7 15 2	−16 12·6
23.	U Persei	1 53 4	+54 20·8	80.	V Geminorum...	7 17 41	+13 16·8
24.	S Arietis	1 59 22	+12 3·4	81.	U Monocerotis..	7 26 7	− 9 34·3
25.	R Arietis.........	2 10 32	+24 36·0	82.	Z Puppis	7 26 24	−20 27·2
26.	o Ceti	2 14 24	− 3 25·3	83.	S Canis Min. ...	7 27 24	+ 8 31·7
27.	R Ceti	2 21 1	− 0 37·2	84.	T Canis Min. ...	7 28 33	+11 57·3
28.	S Horologii......	2 22 26	−60 0·7	85.	X Puppis	7 28 32	−20 42·0
29.	R Fornacis	2 24 52	−26 31·9	86.	U Canis Min. ...	7 36 1	+ 8 36·6
30.	U Ceti............	2 29 2	−13 34·7	87.	S Geminorum ..	7 37 10	+23 40·9
31.	R Trianguli ...	2 31 6	+33 50·3	88.	W Puppis	7 42 43	−41 57·4
32.	T Arietis.........	2 42 51	+17 6·1	89.	T Geminorum ..	7 43 25	+23 58·7
33.	W Persei	2 43 24	+56 34·6	90.	S^2 Puppis	7 43 35	−41 8·4
34.	R Horologii ...	2 50 37	−50 17·4	91.	U Geminorum .	7 49 18	+22 15·6
35.	T Horologii ...	2 57 44	−51 1·8	92.	V Puppis	7 55 26	−48 58·7
36.	Algol	3 1 47	+40 34·7	93.	U Puppis	7 56 14	−12 34·2
37.	U Arietis	3 5 37	+14 25·7	94.	R^2 Puppis	8 8 19	−34 16·9
38.	X Ceti............	3 14 26	− 1 25·5	95.	Y Puppis	8 8 55	−34 50·7
39.	R Persei	3 23 49	+35 20·0	96.	R Cancri.........	8 11 10	+12 1·7
40.	U Eridani	3 46 20	−25 15·2	97.	V Cancri.........	8 16 8	+17 35·8
41.	T Eridani	3 51 2	−24 19·2	98.	V Carinæ	8 26 44	−59 47·7
42.	λ Tauri	3 55 15	+12 12·9	99.	X Carinæ	8 29 9	−58 53·6
43.	R Tauri	4 22 56	+ 9 56·6	100.	U Cancri.........	8 30 10	+19 14·0
44.	S Tauri	4 23 50	+ 9 43·8	101.	T Velorum	8 34 30	−47 1·1
45.	T Camelopardi .	4 30 33	+65 57·0	102.	S Cancri	8 38 21	+19 23·2
46.	R Reticuli	4 32 31	−63 13·9	103.	R Mali	8 41 22	−27 50·6
47.	R Doradûs	4 35 37	−62 16·2	104.	S Hydræ	8 48 27	+ 3 26·3
48.	R Cæli	4 37 6	−38 25·6	105.	T Hydræ.........	8 50 54	− 8 46·0
49.	V Tauri	4 46 21	+17 22·4	106.	T Cancri	8 51 5	+20 13·5
50.	R Orionis	4 53 41	+ 7 59·0	107.	S Mali............	9 0 46	−24 41·9
51.	R Leporis	4 55 9	−14 57·2	108.	W Cancri	9 4 10	+25 38·9
52.	T Leporis	5 0 40	−22 2·2	109.	V Velorum	9 19 18	−55 32·3
53.	V Orionis	5 0 54	+ 3 58·1	110.	S Antliæ	9 28 1	−28 11·8
54.	S Pictoris	5 8 21	−48 37·6	111.	S Velorum	9 29 32	−44 46·5
55.	R Aurigæ	5 9 23	+53 28·6	112.	U Velorum......	9 29 32	−45 4·8
56.	T Columbæ......	5 15 42	−33 48·6	113.	R Carinæ	9 29 47	−62 21·3
57.	W Aurigæ	5 20 17	+36 49·0	114.	X Hydræ	9 30 49	−14 15·2

Mean Places of Variable Stars for 1902·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
115.	R Leo. Min. ...	9 39 43	+34 57·8	173.	δ Libræ	14 55 44	− 8 7·8
116.	Z Hydræ.........	9 40 30	−23 34·1	174.	T^2 Libræ.........	15 0 54	−18 21·2
117.	R^2 Hydræ	9 40 30	−23 34·1	175.	T Libræ	15 5 9	−19 38·7
118.	R Leonis	9 42 18	+11 53·0	176.	Y Libræ	15 6 30	− 5 38·4
119.	*l* Carinæ.........	9 42 33	−62 3·4	177.	R Triang. Austr.	15 11 0	−66 8·2
120.	V Leonis	9 54 35	+21 43·8	178.	U Coronæ	15 14 12	+32 0·3
121.	S Carinæ.........	10 6 15	−61 4·2	179.	S Libræ	15 15 45	−20 2·0
122.	Z Carinæ	10 10 31	−58 22·1	180.	S Serpentis......	15 17 4	+14 39·9
123.	W Velorum ...	10 11 35	−53 59·5	181.	S Coronæ	15 17 24	+31 43·2
124.	Y Carinæ	10 29 29	−57 59·6	182.	S^2 Libræ.........	15 18 36	−22 33·7
125.	R Ursæ Maj. ...	10 37 43	+69 17·4	183.	U^2 Libræ.........	15 27 48	−14 59·7
126.	S^2 Hydræ	10 46 39	−28 6·8	184.	X Libræ	15 30 33	−20 50·3
127.	V Hydræ	10 47 52	−20 43·8	185.	W Libræ.........	15 32 19	−15 51·0
128.	W Leonis	10 48 28	+14 14·3	186.	S Ursæ Minoris.	15 33 22	+78 57·9
129.	U Carinæ	10 53 48	−59 12·5	187.	U Normæ	15 34 46	−54 59·7
130.	S Leonis	11 5 47	+ 6 59·6	188.	U Libræ	15 36 20	−20 51·9
131.	S^2 Centauri......	11 16 11	−61 20·1	189.	T Normæ	15 36 30	−54 40·4
132.	X Centauri	11 44 18	−44 12·6	190.	Z Libræ	15 40 48	−20 49·0
133.	W Centauri ...	11 50 8	−58 42·5	191.	V Coronæ	15 46 1	+39 51·9
134.	R Comæ	11 59 14	+19 19·9	192	R Serpentis......	15 46 11	+15 25·9
135.	S Muscæ	12 7 30	−69 36·4	193.	R Lupi	15 47 7	−36 0·3
136.	T Virginis	12 9 35	− 5 29·5	194.	R Libræ	15 48 3	−15 56·7
137.	R Corvi	12 14 33	−18 42·6	195.	R^2 Libræ.........	15 50 46	−18 1·0
138.	T Crucis	12 16 0	−61 44·3	196.	S Triang. Austr.	15 52 23	−63 29·8
139.	R Crucis	12 18 15	−61 5·1	197	U Triang. Austr.	15 58 36	−62 38·6
140.	T Canum Ven. .	12 25 21	+32 2·8	198.	Z^2 Scorpii	15 58 44	−23 49·8
141.	U Centauri ...	12 28 6	−54 7·2	199.	X Herculis	15 59 43	+47 30·4
142.	Y Virginis	12 28 49	− 3 52·6	200.	Z Scorpii.........	16 0 15	−21 28·0
143.	T Ursæ Maj. ...	12 31 56	+60 1·6	201.	R Herculis	16 1 49	+18 28·1
144.	R Virginis	12 33 32	+ 7 31·7	202.	X Scorpii	16 2 47	−21 15·8
145.	R Muscæ.........	12 36 5	−68 52·2	203.	W Scorpii	16 6 2	−19 52·9
146.	S Ursæ Maj. ...	12 39 40	+61 37·8	204.	U^2 Herculis ...	16 6 8	+25 19·6
147.	U^2 Virginis......	12 42 19	+ 4 40·8	205.	S Normæ	16 10 44	−57 39·5
148.	U Virginis	12 46 7	+ 6 5·2	206.	R Scorpii	16 11 48	−22 42·2
149.	S Crucis	12 48 34	−57 53·9	207.	S Scorpii.........	16 11 50	−22 39·1
150.	W Virginis......	13 20 59	− 2 52·2	208.	W Ophiuchi ...	16 16 8	− 7 27·9
151.	V Virginis	13 22 45	− 2 39·9	209	V Ophiuchi ...	16 21 16	−12 12·3
152.	R Hydræ	13 24 22	−22 46·5	210.	U Herculis	16 21 28	+19 6·9
153.	S Virginis	13 27 53	− 6 41·5	211.	Y Scorpii	16 23 56	−19 13·6
154.	T Centauri	13 36 9	−33 6·1	212.	T Ophiuchi......	16 28 8	−15 55·4
155.	T^2 Centauri ..	13 42 37	−36 22·4	213.	S Ophiuchi......	16 28 37	−16 57·3
156.	W Hydræ	13 43 30	−27 52·6	214.	R Aræ............	16 31 36	−56 47·9
157.	R Canum Ven. .	13 44 45	+40 1·8	215.	W Herculis......	16 31 45	+37 32·4
158.	R^2 Virginis......	13 59 41	− 8 43·7	216.	R Draconis ...	16 32 24	+66 57·5
159.	Z Virginis	14 5 4	−12 50·4	217.	R^2 Ophiuchi ...	16 43 18	−19 17·5
160.	S^2 Hydræ	14 5 55	−28 25·3	218.	S Herculis	16 47 26	+15 6·4
161.	R Centauri	14 9 30	−59 27·4	219.	S^2 Scorpii	16 48 31	−44 56·5
162.	R^2 Centauri......	14 10 3	−57 23·3	220.	R^2 Scorpii	16 50 23	−30 25·4
163.	Y Boötis	14 17 28	+20 15·2	221.	V^2 Scorpii	16 51 55	−33 27·4
164.	S Boötis	14 19 36	+54 15·4	222.	V^2 Herculis......	16 56 49	+31 22·1
165.	S^2 Virginis	14 22 22	+ 5 7·1	223.	R Ophiuchi.....	17 2 8	−15 57·7
166.	R Camelopardi .	14 24 57	+84 16·6	224.	T^2 Herculis......	17 6 51	+27 10·6
167.	V Centauri	14 25 31	−56 27·2	225.	W^2 Scorpii	17 8 26	−33 19·2
168.	V Boötis	14 25 48	+39 17·9	226	U Ophiuchi......	17 11 33	+ 1 19.1
169.	R Boötis	14 32 52	+27 9·7	227.	Z Ophiuchi......	17 14 34	+ 1 37·2
170.	V Libræ	14 34 54	−17 14·1	228.	S^2 Herculis......	17 17 36	+23 1·0
171.	S Lupi............	14 46 52	−46 12·7	229.	S Octantis	17 26 47	−86 46·1
172.	*U Boötis*	14 49 48	+18 5·6	230.	U^2 Scorpii	17 35 21	−43 42·1

Mean Places of Variable Stars for 1902·0 (continued).

No.	Star.	R.A.			Dec.		No.	Star.	R.A.			Dec.	
		h	m	s	°	′			h	m	s	°	′
231.	X Sagittarii ...	17	41	23	−27	47·6	288.	S^2 Aquilæ	19	53	49	− 8	8·9
232.	Y^2 Scorpii	17	41	44	−31	40·0	289.	Z Cygni	19	58	41	+49	46·2
233.	S^3 Scorpii	17	44	24	−33	40·6	290.	S Cygni	20	3	27	+57	42·2
234.	Y Ophiuchi......	17	47	23	− 6	7·2	291.	U^3 Cygni.........	20	3	53	+46	0·9
235.	S Aræ	17	51	37	−49	25·3	292.	R Capricorni ...	20	5	49	−14	33·6
236.	Z Herculis	17	53	41	+15	8·4	293.	S Aquilæ.........	20	7	7	+15	19·7
237.	T Draconis	17	54	54	+58	13·6	294.	R Telescopii ...	20	7	51	−47	7·6
238.	W Sagittarii ...	17	58	46	−29	35·2	295.	U^2 Aquilæ	20	8	9	+12	42·1
239.	R Pavonis	18	3	28	−63	38·2	296.	r^2 Aquilæ	20	8	9	+12	42·0
240.	T Herculis	18	5	24	+31	0·1	297.	W Capricorni...	20	8	43	−22	16·6
241.	S^2 Sagittarii ...	18	11	6	−34	8·5	298.	R Sagittæ	20	9	35	+16	25·8
242.	W Lyræ	18	11	32	+36	38·4	299.	Z Aquilæ.........	20	9	58	− 6	27·0
243.	Y Sagittarii......	18	15	37	−18	54·3	300.	R Delphini......	20	10	11	+ 8	47·5
244.	V^2 Sagittarii ...	18	21	29	−33	22·8	301.	T^2 Sagittarii ...	20	11	13	−39	24·9
245.	*d* Serpentis	18	22	12	+ 0	8·3	302.	U Cygni	20	16	34	+47	35·1
246.	T Serpentis......	18	24	2	+ 6	13·9	303.	X^3 Cygni	20	19	41	+42	55·3
247.	U Sagittarii ...	18	26	7	−19	11·8	304.	Y^3 Cygni	20	29	42	+46	16·0
248.	X Ophiuchi ...	18	33	41	+ 8	44·5	305.	S^3 Cygni	20	29	59	+54	38·0
249.	X Lyræ	18	34	16	+43	51·9	306.	W Delphini	20	33	12	+17	56·3
250.	U Coron. Austr.	18	34	25	−37	55·5	307.	R Microscopii...	20	34	5	−29	8·2
251.	R Scuti	18	42	15	− 5	48·7	308.	S Capricorni ...	20	36	8	−19	24·3
252.	β Lyræ	18	46	28	+33	14·9	309.	V Cygni	20	38	9	+47	47·4
253.	κ Pavonis	18	46	51	−67	21·4	310.	S Delphini	20	38	34	+16	44·1
254.	R Lyræ	18	52	21	+43	49·0	311.	Y Aquarii	20	39	15	− 5	11·6
255.	R Coron. Austr.	18	55	17	−37	5·5	312.	X Cygni	20	39	34	+35	13·9
256.	Y Lyræ	18	56	4	+34	49·2	313.	T Delphini	20	40	49	+16	2·5
257.	R Aquilæ	19	1	39	+ 8	4·9	314.	W Aquarii	20	41	17	− 4	26·5
258.	V Lyræ	19	5	14	+29	30·1	315.	V Aquarii	20	41	52	+ 2	4·7
259.	X^2 Sagittarii ...	19	8	49	−18	58·6	316.	U Capricorni ...	20	42	41	−15	8·6
260.	S Lyræ	19	9	11	+25	50·2	317.	R^2 Cygni.........	20	42	41	+44	30·6
261.	Z Lyræ	19	9	22	+33	14·8	318.	V Delphini......	20	43	19	+18	58·5
262.	U Draconis	19	9	57	+67	7·1	319.	T Aquarii	20	44	46	− 5	30·6
263.	W Aquilæ	19	10	6	− 7	12·8	320.	U Pavonis	20	47	22	−63	4·8
264.	T Sagittarii ...	19	10	35	−17	8·6	321.	T Vulpeculæ ...	20	47	19	+27	52·7
265.	R Sagittarii ...	19	10	57	−19	28·8	322.	Y Cygni	20	48	9	+34	17·4
266.	S Sagittarii......	19	13	42	−19	12·2	323.	Z^2 Cygni	20	48	36	+46	59·2
267.	Z Sagittarii......	19	13	54	−21	6·4	324.	S Indi	20	49	8	−54	41·9
268.	U Lyræ	19	16	41	+37	41·8	325.	X Delphini......	20	50	23	+17	16·1
269.	T Sagittæ	19	17	20	+17	28·1	326.	R^2 Capricorni..	20	56	30	−27	28·5
270.	U Aquilæ	19	24	5	− 7	14·7	327.	T Octantis	20	57	45	−82	29·5
271.	U Vulpeculæ ...	19	32	21	+20	6·9	328.	R Vulpeculæ ..	21	0	2	+23	26·0
272.	T^2 Aquilæ	19	33	23	+11	29·2	329.	V Capricorni ...	21	1	55	−24	19·2
273.	R Cygni	19	34	11	+49	58·8	330.	X Capricorni ..	21	2	57	−21	44·5
274.	T Pavonis	19	39	44	−72	0·8	331.	X Cephei	21	3	30	+82	40·3
275.	T^3 Cygni.........	19	40	53	+29	0·7	332.	Z Capricorni ...	21	5	10	−16	34·7
276.	T^2 Cygni.........	19	40	53	+48	32·2	333.	S^2 Aquarii	21	5	51	− 4	25·6
277.	V^3 Cygni.........	19	42	48	+32	27·9	334.	T Cephei.........	21	8	15	+68	5·5
278.	W^3 Cygni	19	43	22	+48	49·6	335.	R^2 Aquarii	21	9	55	− 3	18·1
279.	S Vulpeculæ ...	19	44	23	+27	2·5	336.	X Pegasi	21	16	20	+14	1·8
280.	X Aquilæ.........	19	46	37	+ 4	12·8	337.	T Capricorni ..	21	16	37	−15	34·4
281.	χ Cygni	19	46	48	+32	40·0	338.	S Microscopii...	21	20	55	−30	16·4
282.	S Pavonis	19	46	57	−59	26·9	339.	Y Capricorni ...	21	29	2	−14	24·5
283.	η Aquilæ.........	19	47	29	+ 0	45·2	340.	W Cygni.........	21	32	21	+44	56·2
284.	R^2 Sagittarii ...	19	49	51	−29	26·7	341.	S Cephei	21	36	27	+78	11·0
285.	S Sagittæ	19	51	33	+16	22·4	342.	U^2 Cygni.........	21	37	20	+53	52·8
286.	U^2 Sagittarii ...	19	51	59	−42	6·6	343.	R^3 Cygni.........	21	38	51	+43	8·1
287.	R^2 Aquilæ	19	52	31	− 2	8·8	344.	R Gruis	21	42	13	−47	22·0

Mean Places of Variable Stars for 1902·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
345.	V Pegasi	21 56 8	+ 5 39·0	359.	V Cassiopeiæ ...	23 7 28	+59 9·1
346.	U Aquarii	21 57 59	−17 5·9	360.	W Pegasi	23 14 52	+25 44·2
347.	S Piscis Austr. .	21 58 9	−28 31·4	361.	S Pegasi	23 15 35	+ 8 23·0
348.	T Pegasi	22 4 6	+12 3·6	362	R Aquarii	23 38 45	−15 49·6
349.	R Piscis Austr. .	22 12 26	−30 5·6	363.	R Phœnicis......	23 51 22	−50 19·9
350.	X Aquarii	22 13 16	−21 23·4	364.	V Cephei.........	23 51 49	+82 38·7
351.	T Gruis	22 19 58	−38 3·8	365.	R Toucani	23 52 18	−65 55·7
352.	S Gruis	22 20 3	−48 55·8	366.	V Ceti	23 52 53	− 9 30·4
353.	S Lacertæ	22 24 43	+39 48·9	367.	U Pegasi.........	23 52 58	+15 24·6
354.	δ Cephei	22 25 32	+57 54·8	368.	R Cassiopeiæ ...	23 53 25	+50 50·6
355.	W Cephei	22 32 43	+57 55·3	369.	S Phœnicis	23 54 0	−57 7·2
356.	R Lacertæ	22 38 55	+41 51·5	370	W Ceti	23 57 6	−15 13·3
357.	S Aquarii	22 51 51	−20 52·0	371.	Y Cassiopeiæ ...	23 58 16	+55 8·0
358.	R Pegasi	23 1 44	+10 0·9				

MAXIMA OF U PEGASI.

Jan.	April.	June.	August.	Oct.	Dec.
d h m	d h m	d h m	d h m	d h m	d h m
1 4 0	4 20 42	3 19 52	2 19 2	1 18 13	4 11 20
4 21 57	8 14 39	7 13 49	6 12 59	5 12 10	8 5 17
8 15 53	12 8 36	11 7 46	10 6 56	9 6 6	11 23 14
12 9 50	16 2 33	15 1 43	14 0 53	13 0 3	15 17 10
16 3 47	19 20 29	18 19 40	17 18 50	16 18 0	19 11 7
19 21 46	23 14 26	22 13 37	21 12 47	20 11 57	23 5 4
23 15 41	27 8 23	26 7 33	25 6 44	24 5 54	26 23 1
27 9 38		30 1 30	29 0 41	27 23 51	30 15 58
31 3 35				31 17 48	
Feb.	May.	July.	Sept.	Nov.	
	1 2 20				
3 21 32	4 20 17	3 19 27	1 18 38	4 11 45	
7 15 29	8 14 14	7 13 26	5 12 34	8 5 42	
11 9 25	12 8 11	11 7 21	9 6 31	11 23 38	
15 3 22	16 2 8	15 1 18	13 0 28	15 17 35	
18 21 19	19 20 5	18 19 15	16 18 25	19 11 32	
22 15 16	23 14 1	22 13 12	20 12 22	23 5 29	
26 9 13	27 7 58	26 7 9	24 6 19	26 23 26	
	31 1 55	30 1 5	28 0 16	30 17 23	

Every twentieth minimum is given; the others may be found by adding multiples of the period $4^{d}\ 29^{m}\ 8^{s}$.

The minimum takes place $2^{h}\ 15^{m}$ after the maximum.

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's name refers to Catalogue on pp. 23–26.)

te.—The times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

ONOCER. 68.

(7ᵈ 22½ʰ.)

May		Sept.	
d	h	d	h
23	1	8	2
June		Oct.	
19	1	5	2
July		Nov.	
16	1	1	3
		28	3
Aug.		Dec.	
12	2	25	3

EMINOR. 69.

(2ᵈ 15½ʰ.)

Apr.		Oct.	
d	h	d	h
18	3	5	10
25	21	13	4
		20	22
May		28	15
3	15		
11	8	Nov.	
		5	c
Aug.		13	3
		20	21
4	12	28	14
12	6		
19	23		
27	17	Dec.	
		6	8
Sept.		14	2
4	11	21	20
12	5	29	13
19	22		
27	16		

EMINOR. 74.

(5ᵈ 0ʰ.)

Feb.		Apr.	
d	h	d	h
23	9	5	0
		15	4
Mar.		25	7
5	13		
15	17		
25	20		

ζ Geminor. 74

(*continued*).

May		Sept.		Nov.	
d	h	d	h	d	h
5	11	4	7	4	6
15	15	14	11	14	9
25	19	24	15	24	13
Aug.		Oct.		Dec.	
15	0	4	19	4	17
25	4	14	22	14	20
		25	2	25	0

W Virginis 150.

(8ᵈ 4½ʰ.)

Jan.		May		Sept.	
d	h	d	h	d	h
7	13	8	10	6	8
24	19	25	17	23	14
Feb.		June		Oct.	
11	2	11	23	10	21
28	8	29	6	28	3
Mar.		July		Nov.	
17	15	20	4	14	10
Apr.		Aug.		Dec.	
3	21	2	19	1	16
21	4	20	1	18	23

X Sagittar. 231.

(2ᵈ 21ʰ.)

Feb.		May		Sept.	
d	h	d	h	d	h
7	20	17	0	6	5
14	21	24	1	13	5
21	21	31	1	20	5
28	21			27	6
		June			
Mar.		7	1	Oct.	
7	21	14	1	4	6
14	22	21	2	11	6
21	22	28	2	18	6
28	22	July		25	7
		5	2		
Apr.		12	3	Nov.	
4	23	19	3	1	7
11	23	26	3	8	7
18	23			15	8
25	23	Aug.		22	8
		2	3	29	8
May		9	4		
		16	4	Dec.	
3	0	23	4		
10	0	30	5	6	8

Y Ophiuchi 234.

(6ᵈ 5ʰ.)

Jan.		Apr.		Aug.	
d	h	d	h	d	h
9	9	22	3	2	20
26	12			19	23
		May			
Feb.		9	6		
12	15	26	8	Sept.	
				6	2
Mar.		June		23	5
1	18	12	11		
18	21	29	14		
				Oct.	
Apr.		July		10	8
5	0	16	17	27	10

W Sagittar. 238.

(3ᵈ 0ʰ.)

Jan.		May		Sept.	
d	h	d	h	d	h
30	13	1	17	7	2
		9	7	15	10
		16	21	23	0
Feb.		24	12	30	14
7	4	June			
14	18				
22	8	1	2		
		8	16	Oct.	
		16	6	8	4
Mar.		23	21	15	19
		July		23	9
1	23			30	23
9	13	1	11		
17	3	9	1		
24	17	16	15		
		24	6	Nov.	
		31	20	7	13
Apr.		Aug.		15	4
1	8	8	10	22	18
8	22	16	0		
16	12	23	15		
24	2	31	5		

Y Sagittar. 243.

(1ᵈ 19ʰ.)

Jan.		May		Aug.	
d	h	d	h	d	h
23	18	1	21	31	3
29	12	7	16		
		13	10	Sept.	
Feb.		19	5	5	21
		24	23	11	16
4	7	30	18	17	10
10	1			23	5
15	20	June		28	23
21	14	5	12		
27	9	11	7	Oct.	
		17	1	4	18
		22	20	10	13
Mar.		28	15	16	7
5	3	July		22	2
10	22			27	20
16	17	4	9		
22	11	10	4	Nov.	
28	6	15	22	2	15
		21	17	8	9
		27	11	14	4
Apr.		Aug.		19	22
3	0	2	6	25	17
8	19	8	0		
14	13	13	19	Dec.	
20	8	19	14	1	12
26	2	25	8		

U Sagittar. 247.

(2ᵈ 23½ʰ.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
19	1	7	6	16	18
25	19	14	0	23	12
		20	18	30	5
Feb.		27	12		
				May	
1	13				
8	7	Apr.		6	23
15	1			13	17
21	19	3	6	20	11
28	13	10	0	27	5

U Sagittar. 247

(*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
2	23	2	16	2	8
9	17	9	9	9	2
16	11	16	3	15	20
23	4	22	21	22	14
29	22	29	15	29	8
July		Sept.		Nov.	
6	16	5	9	5	2
13	10	12	3	11	20
20	4	18	21	18	14
26	22	25	15		

β Lyræ 252.

(Max. 3ᵈ 2ʰ.
Secondary Min.
6ᵈ 11ʰ.
Max. 9ᵈ 20ʰ.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	21	11	1	4	7
14	19	23	23	17	5
27	17			30	3
Feb.		June		Oct.	
9	15	5	21	13	1
22	13	18	19	25	23
Mar.		July		Nov.	
7	11	1	17	7	21
20	9	14	15	20	19
		27	13		
Apr.		Aug.		Dec.	
2	7	9	11	3	17
15	5	22	9	16	15
28	3				

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 270.

(2^d 4½^h.)

Feb.		May		Sept.	
d	h	d	h	d	h
6	16	23	1	5	10
13	17	30	1	12	10
20	17	June		19	11
27	18	6	2	26	11
Mar.		13	3	Oct.	
6	19	20	3	3	12
13	19	27	4	10	12
20	20	July		17	13
27	20	4	4	24	14
Apr.		11	5	31	14
3	21	18	5	Nov.	
10	21	25	6	7	15
17	22	Aug.		14	15
24	23	1	7	21	16
May		8	7	28	16
1	23	15	8		
9	0	22	8		
16	0	29	9		

η AQUILÆ 283.

(2^d 6^h.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
13	7	6	20	4	12
20	11	14	0	11	17
27	15	21	4	18	21
		28	8	26	1

η AQUILÆ 283 (*continued*).

May		July		Oct.	
d	h	d	h	d	h
3	5	21	4	8	2
10	10	28	8	15	7
17	14	Aug.		22	11
24	18	4	12	29	15
31	22	11	17	Nov.	
June		18	21	5	19
8	3	26	1	13	0
15	7	Sept.		20	4
22	11	2	5	27	8
29	15	9	10	Dec.	
July		16	14	4	12
6	19	23	18	11	17
14	0	30	22	18	21
				26	1

S SAGITTÆ 285.

(3^d 10^h.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
20	22	3	20	23	3
29	7	12	5	May	
Feb.		20	15	1	13
6	17	29	0	9	22
15	2	Apr.		18	7
23	11	6	9	26	16
		14	18		

S SAGITTÆ 285 (*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
4	1	1	18	7	19
12	11	10	3	16	5
20	20	18	12	24	14
29	5	26	21	Nov.	
July		Sept.		1	23
7	14	4	7	10	8
15	23	12	16	18	17
24	9	21	1	27	3
		29	10	Dec.	
				5	12
				13	21

X CYGNI 312.

(6^d 19^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
5	16	16	18	8	11
22	1	June		24	20
Feb.		2	4	Oct.	
7	11	18	13	11	6
23	20	July		27	15
Mar.		4	22	Nov.	
12	5	21	7	13	0
28	14	Aug.		29	9
Apr.		6	17	Dec.	
14	0	23	2	15	19
30	9				

T VULPEC. 321.

(1^d 9½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	3	1	22	3	2
6	14	6	8	7	13
11	0	10	19	11	23
15	11	15	5	16	10
19	21	19	15	20	20
24	7	24	2	25	7
28	18	28	12	29	17
Feb.		June		Oct.	
2	4	1	23	4	4
6	15	6	9	8	14
11	1	10	20	13	1
15	12	15	6	17	11
19	22	19	17	21	21
24	9	24	3	26	8
28	19	28	14	30	18
Mar.		July		Nov.	
5	6	3	0	4	5
9	16	7	10	8	15
14	2	11	21	13	2
18	13	16	7	17	12
22	23	20	18	21	23
27	10	25	4	26	9
31	20	29	15	30	20
Apr.		Aug.		Dec.	
5	7	3	1	5	6
9	17	7	12	9	17
14	4	11	22	14	3
18	14	16	9	18	13
23	1	20	19	23	0
27	11	25	6	27	10
		29	16	31	21

δ CEPHEI

(1^d 15

Jan.		May	
d	h	d	
2	2	5	1
7	10	10	2
12	19	16	
18	4	21	1
23	13	26	2
28	21		
Feb.		June	
3	6	1	8
8	15	6	17
14	0	12	1
19	9	17	10
24	18	22	19
		28	4
Mar.		July	
2	2	3	1
7	11	8	21
12	20	14	6
18	5	19	15
23	14	25	0
28	22	30	9
Apr.		Aug.	
3	7	4	17
8	16	10	2
14	1	15	11
19	10	20	20
24	18	26	5
30	3	31	13

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following Star's name are the Nos. in Catalogue, pp. 23–26.)

U Cephei 14 *.

$(2^{d}\ 11^{h}\ 49\frac{1}{2}^{m}.)$

d	h	m
Jan.		
3	6	16
8	5	55
13	5	34
18	5	13
23	4	53
28	4	32
Feb.		
2	4	11
7	3	50
12	3	29
17	3	8
22	2	47
27	2	26
March.		
4	2	5
9	1	44
14	1	23
19	1	2
24	0	41
29	0	21
April.		
3	0	0
7	23	39
12	23	18
17	22	57
22	22	36
27	22	15
May.		
2	21	54
7	21	33
12	21	12
17	20	51
22	20	30
27	20	9
June.		
1	19	48
6	19	28
11	19	7
16	18	46
21	18	25
26	18	4

d	h	m
July.		
1	17	43
6	17	22
11	17	1
16	16	40
21	16	19
26	15	58
31	15	37
August.		
5	15	16
10	14	56
15	14	35
20	14	14
25	13	53
30	13	32
Sept.		
4	13	11
9	12	50
14	12	29
19	12	8
24	11	47
29	11	26
Oct.		
4	11	5
9	10	44
14	10	24
19	10	3
24	9	42
29	9	21
Nov.		
3	9	0
8	8	39
13	8	18
18	7	57
23	7	36
28	7	15
Dec.		
3	6	54
8	6	33
13	6	12
18	5	51
23	5	31
28	5	10

Algol 36 *.

$(2^{d}\ 20^{h}\ 49^{m}.)$

d	h	m
Jan.		
3	2	40
8	20	18
14	13	56
20	7	34
26	1	12
31	18	49
Feb.		
6	12	27
12	6	5
17	23	43
23	17	21
March.		
1	10	59
7	4	37
12	22	14
18	15	52
24	9	30
30	3	8
April.		
4	20	46
10	14	24
16	8	1
22	1	39
27	19	17
June.		
27	0	25
July.		
2	18	2
8	11	40
14	5	18
19	22	56

d	h	m
July.		
25	16	34
31	10	12
August.		
6	3	49
11	21	27
17	15	5
23	8	43
29	2	21
Sept.		
3	19	59
9	3	37
15	7	14
21	0	52
26	18	30
Oct.		
2	12	8
8	5	46
13	23	24
19	17	1
25	10	39
31	4	17
Nov.		
5	21	55
11	15	33
17	9	11
23	2	49
28	20	26
Dec.		
4	14	4
10	7	42
16	1	20
21	18	58
27	12	36

λ Tauri 42 *.

$(3^{d}\ 22^{h}\ 52^{m}.)$

d	h	m
Jan.		
1	18	57
9	16	42
17	14	26
25	12	11
Feb.		
2	9	55
10	7	39
18	5	24
26	3	8

d	h	m
March.		
6	0	53
13	22	37
21	20	21
29	18	6
April.		
6	15	50
14	13	35
22	11	19
30	9	3

λ Tauri 42 *.

(*continued*).

d	h	m
July.		
2	14	59
10	12	43
18	10	27
26	8	12
August.		
3	5	56
11	3	41
19	1	25
26	23	9
Sept.		
3	20	54
11	18	38
19	16	23
27	14	7

d	h	m
Oct.		
5	11	51
13	9	36
21	7	20
29	5	5
Nov.		
6	2	49
14	0	33
21	22	18
29	20	2
Dec.		
7	17	47
15	15	31
23	13	15
31	11	0

R Canis Maj. 79 †.

$\left\{\begin{array}{l}1=1^{d}\ 3^{h}\ 16^{m}\\ 2=2\ \ 6\ \ 31\end{array}\right\}$

d	h	m
Jan.		
1	14	58
5	0	46
8	10	33
11	20	20
15	6	8
18	15	55
22	1	42
25	11	30
28	21	17
Feb.		
1	7	4
4	16	51
8	2	39
11	12	26
14	22	13
18	8	1
21	17	48
25	3	35
28	13	23
March.		
3	23	10
7	8	57
10	18	44
14	4	32

d	h	m
March.		
17	14	19
21	0	6
24	9	54
27	19	41
31	5	28
April.		
3	15	16
7	1	3
10	10	50
13	20	27
17	6	25
20	16	12
24	1	59
27	11	47
30	21	34
May.		
4	7	21
7	17	9
11	2	56
14	12	43
17	22	30
21	8	18
24	8	15
28	3	52
31	13	40

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 79 †
(*continued*).

Sept.

d	h	m
1	17	13
5	3	0
8	12	47
11	22	34
15	8	22
18	18	9
22	3	56
25	13	44
28	23	31

Oct.

d	h	m
2	9	18
5	19	6
9	4	53
12	14	40
16	0	27
19	10	15
22	20	2
26	5	49
29	15	37

Nov.

d	h	m
2	1	24
5	11	11
8	20	59
12	6	46
15	16	33
19	2	20
22	12	8
25	21	55
29	7	42

Dec.

d	h	m
2	17	30
6	3	17
9	13	4
12	22	52
16	8	39
19	18	26
23	4	13
26	14	1
29	23	48

S Cancri 102.

Jan.

d	h	m
9	3	27
18	15	4
28	2	42

Feb.

d	h	m
6	14	20
16	1	58
25	13	35

March.

d	h	m
7	1	13
16	12	51
26	0	29

April.

d	h	m
4	12	6
13	23	44
23	11	22

May.

d	h	m
2	23	0
12	10	37
21	22	15
31	9	53

June.

d	h	m
9	21	31
18	9	8
28	20	46

July.

d	h	m
8	8	24
17	20	2
27	7	39

Sept.

d	h	m
3	6	10
12	17	48
22	5	26

Oct.

d	h	m
1	17	4
11	4	41
20	16	19
30	3	57

Nov.

d	h	m
8	15	35
18	3	12
27	14	50

Dec.

d	h	m
7	2	28
16	14	6
26	1	43

S Antliæ 110 §.

Jan.

d	h	m
1	1	12
4	7	0
7	12	48
10	18	36
14	0	24
17	6	12
20	12	0
23	17	48
26	23	36
30	5	33

Feb.

d	h	m
2	11	12
5	17	0
8	22	48
12	4	36
15	10	24
18	16	12
21	22	0
25	3	48
28	9	36

March.

d	h	m
3	15	24
6	21	12
10	3	0
13	8	48
16	14	36
19	20	24
23	2	12
26	8	0
29	13	48

April.

d	h	m
1	19	36
5	1	24
8	7	12
11	13	0
14	18	48
18	0	36
21	6	24
24	12	12
27	18	0
30	23	48

May.

d	h	m
4	5	36
7	11	24
10	17	12
13	23	0
17	4	48
20	10	36
23	16	24
26	22	12
30	4	0

June.

d	h	m
2	9	48
5	15	36
8	21	24
12	3	12
15	9	0
18	14	48
21	20	36
25	2	24
28	8	12

July.

d	h	m
1	14	0
4	19	48
8	1	36
11	7	24
14	13	12
17	19	0
21	0	48
24	6	36
27	12	24

Sept.

d	h	m
4	10	0
7	15	48
10	21	36
14	3	24
17	9	12
20	15	0
23	20	48
27	2	36
30	8	24

Oct.

d	h	m
3	14	12
6	20	0
10	1	48
13	7	36
16	13	24
19	19	12
23	1	0
26	6	48
29	12	36

Nov.

d	h	m
1	18	24
5	0	12
8	6	0
11	11	48
14	17	36
17	23	24
21	5	12
24	11	0
27	16	48
30	22	36

S Antliæ 110 §
(*continued*).

Dec.

d	h	m
4	4	24
7	10	12
10	16	0
13	21	48
17	3	36

Dec.

d	h	m
20	9	24
23	15	12
26	21	0
30	2	48

δ Libræ 173 *.
$(2^d\ 7^h\ 51^m.)$

Jan.

d	h	m
2	2	32
6	18	14
11	9	57
16	1	40
20	17	23
25	9	5
30	0	48

Feb.

d	h	m
3	16	31
8	8	14
12	23	56
17	15	39
22	7	22
26	23	5

March.

d	h	m
3	14	47
8	6	30
12	22	13
17	13	56
22	5	38
26	21	21
31	13	4

April.

d	h	m
5	4	47
9	20	30
14	12	12
19	3	55
23	19	38
28	11	21

May.

d	h	m
3	3	3
7	18	46
12	10	29
17	2	12
21	17	54
26	9	37
31	1	20

June.

d	h	m
4	17	3
9	8	45
14	0	28
18	16	11
23	7	54
27	23	36

July.

d	h	m
2	15	19
7	7	2
11	22	45
16	14	27
21	6	10
25	21	53
30	13	44

August.

d	h	m
4	5	19
8	21	1
13	12	44
18	4	27
22	20	10
27	11	52

Sept.

d	h	m
1	3	35
5	19	18
10	11	1
15	2	43
19	18	26
24	10	9
29	1	52

Dec.

d	h	m
3	5	50
7	21	33
12	13	16
17	4	59
21	20	41
26	12	24
31	4	7

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^h\ 46^m\cdot 8$.

Minima of Variable Stars of the Algol Type (continued).

U CORONÆ 178 *.

(3ᵈ 10ʰ 51ᵐ.)

	d	h	m
Jan.	4	2	17
	10	23	59
	17	21	41
	24	19	23
	31	17	5
Feb.	7	14	47
	14	12	29
	21	10	11
	28	7	53
March.	7	5	35
	14	3	17
	21	0	59
	27	22	41
April.	3	20	23
	10	18	5
	17	15	47
	24	13	29
May.	1	11	11
	8	8	53
	15	6	35
	22	4	17
	29	1	59
June.	4	23	41
	11	21	23
	18	19	5
	25	16	47

	d	h	m
July.	2	14	29
	9	12	11
	16	9	53
	23	7	35
	30	5	17
August.	6	2	59
	13	0	41
	19	22	23
	26	20	5
Sept.	2	17	47
	9	15	29
	16	13	11
	23	10	53
	30	8	35
Oct.	7	6	17
	14	3	59
	21	1	41
	27	23	23
Nov.	3	21	5
	10	18	47
	17	16	29
	24	14	11
Dec.	1	11	53
	8	9	35
	15	7	17
	22	4	59
	29	2	41

U OPHIUCHI 226 ‡.

1 = 0ᵈ 20ʰ 8ᵐ
2 = 1 16 15
3 = 2 12 23

	d	h	m
Jan.	0	6	46
	3	15	17
	6	23	48
	10	8	18
	13	16	49
	17	1	20
	20	9	51
	23	18	22

	d	h	m
Jan.	27	2	53
	30	11	24
Feb.	2	19	55
	6	4	26
	9	12	57

U OPHIUCHI 226 ‡ (*continued*).

	d	h	m
Feb.	12	21	28
	16	5	59
	19	14	30
	22	23	1
	26	7	32
March.	1	16	3
	5	0	34
	8	9	5
	11	17	36
	15	2	7
	18	10	38
	21	19	9
	25	3	40
	28	12	11
	31	20	42
April.	4	5	12
	7	13	43
	10	22	14
	14	6	48
	17	15	16
	20	23	47
	24	8	18
	27	16	49
May.	1	1	20
	4	9	51
	7	18	22
	11	2	53
	14	11	24
	17	19	55
	21	4	26
	24	12	57
	27	21	28
	31	5	59
June.	3	14	30
	6	23	1
	10	7	32
	13	16	3
	17	0	34
	20	9	5
	23	17	35
	27	2	6
	30	10	37

	d	h	m
July.	3	19	8
	7	3	39
	10	12	10
	13	20	41
	17	5	12
	20	13	43
	23	22	14
	27	6	45
	30	15	16
August.	2	23	47
	6	8	18
	9	16	49
	13	1	20
	16	9	51
	19	18	22
	23	2	53
	26	11	24
	29	19	55
Sept.	2	4	26
	5	12	57
	8	21	27
	12	5	58
	15	14	29
	18	23	0
	22	7	31
	25	16	2
	29	0	33
Oct.	2	9	4
	5	17	35
	9	2	6
	12	10	37
	15	19	8
	19	3	39
	22	12	10
	25	20	41
	29	5	12
Nov.	1	13	43
	4	22	14
	8	6	45
	11	15	16
	14	23	47
	18	8	18

Z HERCULIS 236 ‡.

1 = 2ᵈ 1ʰ 19ᵐ
2 = 3 23 50
3 = 6 1 8

	d	h	m
Jan.	1	23	21
	9	23	1
	17	22	40
	25	22	19
Feb.	2	21	58
	10	21	37
	18	21	16
	26	20	55
March.	6	20	34
	14	20	13
	22	19	52
	30	19	32
April.	7	19	11
	15	18	50
	23	18	29
May.	1	18	8
	9	17	47
	17	17	26
	25	17	5
June.	2	16	44
	10	16	24
	18	16	3
	26	15	42

	d	h	m
July.	4	15	21
	12	15	0
	20	14	39
	28	14	18
August.	5	13	57
	13	13	36
	21	13	16
	29	12	55
Sept.	6	12	33
	14	12	13
	22	11	52
	30	11	31
Oct.	8	11	10
	16	10	49
	24	10	28
Nov.	1	10	8
	9	9	47
	17	9	26
	25	9	5
Dec.	3	8	44
	11	8	23
	19	8	2
	27	7	4

V³ CYGNI 277 *.

(6ᵈ 0ʰ 9ᵐ.)

	d	h	m
Jan.	6	5	38
	18	5	56
	30	6	14
Feb	11	6	31
	23	6	49
March.	7	7	6
	19	7	24
	31	7	42

	d	h	m
April.	12	7	59
	24	8	17
May.	6	8	34
	18	8	52
	30	9	10
June.	11	9	27
	23	9	45

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

V³ CYGNI 277* (*continued*).

Month	d	h	m	Month	d	h	m
July.	5	10	2	Oct.	9	12	23
	17	10	20		21	12	41
	29	10	38	Nov.	2	12	58
August.	10	10	55		14	13	16
	22	11	13		26	13	34
Sept.	3	11	30	Dec.	8	13	51
	15	11	48		20	14	9
	27	12	6				

U³ CYGNI 291 *.

(4ᵈ 13ʰ 45ᵐ.)

Month	d	h	m	Month	d	h	m
Jan.	2	10	4	June.	6	21	35
	11	13	34		16	1	5
	20	17	4		25	4	35
	29	20	34	July.	4	8	5
Feb.	8	0	4		13	11	35
	17	3	34		22	15	5
	26	7	4		31	18	35
March.	7	10	34	Aug.	9	22	5
	16	14	4		19	1	35
	25	17	34		28	5	5
April.	3	21	4	Sept.	6	8	36
	13	0	34		15	12	6
	22	4	5		24	15	36
May.	1	7	35	Oct.	3	19	6
	10	11	5		12	22	36
	19	14	35		22	2	6
	28	18	5		31	5	36

U³ CYGNI 291 * (*continued*).

Month	d	h	m	Month	d	h	m
Nov.	9	9	6	Dec.	6	19	36
	18	12	36		15	23	6
	27	16	6		25	2	36

W DELPHINI 306 *.

(4ᵈ 19ʰ 21ᵐ.)

Month	d	h	m	Month	d	h	m
Jan.	1	23	28	July.	3	14	54
	11	14	11		13	5	36
	21	4	53		22	20	19
	30	19	36	Aug.	1	11	1
Feb.	9	10	18		11	1	44
	19	1	0		20	16	26
	28	15	43		30	7	8
March.	10	6	25	Sept.	8	21	51
	19	21	8		18	12	33
	29	11	50		28	3	16
April.	8	2	32	Oct.	7	17	58
	17	17	15		17	8	40
	27	7	57		26	23	23
May.	6	22	40	Nov.	5	14	5
	16	13	22		15	4	48
	26	4	4		24	19	30
June.	4	18	47	Dec.	4	10	12
	14	9	29		14	0	55
	24	0	12		23	15	37

Y CYGNI 322 ‡.

1 = 1ᵈ 7ʰ 54ᵐ
2 = 2 23 54
3 = 4 16 49

Month	d	h	m	Month	d	h	m
Jan.	2	7	34	July.	1	2	8
	8	7	23		7	1	58
	14	7	12		13	1	47
	20	7	1		19	1	36
	26	6	50		25	1	25
Feb.	1	6	39		31	1	14
	7	6	29	August.	6	1	3
	13	6	18		12	0	53
	19	6	7		18	0	42
	25	5	56		24	0	31
March.	3	5	45		30	0	20
	9	5	34	Sept.	5	0	9
	15	5	24		10	23	58
	21	5	13		16	23	48
	27	5	2		22	23	37
April.	2	4	51		28	23	26
	8	4	40	Oct.	4	23	15
	14	4	29		10	23	4
	20	4	19		16	22	53
	26	4	8		22	22	42
May.	2	3	57		28	22	32
	8	3	46	Nov.	3	22	21
	14	3	35		9	22	10
	20	3	24		15	21	59
	26	3	13		21	21	48
June.	1	3	3		27	21	37
	7	2	52	Dec.	3	21	27
	13	2	41		9	21	16
	19	2	30		15	21	5
	25	2	19		21	20	54
					27	20	43

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 23–26; M signifies maximum; *m*, minimum.

nuary.

	No.
1	370 M
	171 M
	183 *m*
2	94 M
	107 M
	59 M
3	362 M
	257 *m*
	143 *m*
5	119 M
	140 M
	84 M
	154 *m*
6	248 M
	293 M
	254 M
7	250 M
	19 *m*
8	80 M
	96 M
9	240 M
10	204 M
	189 M
11	222 M
	232 *m*
12	316 M
	262 *m*
13	274 M
14	255 M
	339 M
	32 *m*
15	323 M
	329 M
16	50 M
	71 M
	49 M
17	30 *m*
18	53 M
	309 M
	188 M
	81 *m*
20	118 M
21	165 M
	113 M
22	228 M
	56 M
	363 *m*
23	10 M
	232 M
	8 M
	179 *m*
24	87 M
	184 *m*
	335 *m*
25	130 M
	299 *m*
27	119 *m*
28	357 M
	353 *m*
29	1 M
	278 M
	209 *m*
30	254 *m*
	279 *m*
31	129 *m*
	94 *m*
	44 *m*

February.

d	No.
1	249 M
	289 *m*
	229 *m*
2	295 M
3	266 M
	185 M
	301 M
	26 *m*
	198 *m*
	298 *m*
4	297 M
	86 *m*
5	81 M
	288 M
	129 M
	327 *m*
6	338 *m*
7	157 M
	121 M
8	125 M
9	119 M
11	131 *m*
12	94 M
	239 M
13	328 M
14	12 M
15	155 M
	153 *m*
16	164 M
	142 *m*
	269 *m*
17	201 M
18	174 M
	317 *m*
19	61 M
	123 M
	298 M
	177 *m*
	232 *m*
20	181 M
	154 M
	359 M
	368 *m*
21	342 M
	254 M
	75 *m*
	3 *m*
	147 *m*
22	88 M
	216 M
23	195 M
24	23 M
25	324 M
	279 M
	128 M
	24 *m*
	175 *m*
	307 *m*
26	144 M
	104 M
	141 *m*
27	334 M
	199 M
28	78 M
	174 *m*

March.

d	No.
1	345 M
2	13 M
	25 *m*
3	232 M
	224 *m*
4	273 *m*
	255 *m*
	119 *m*
5	369 M
	81 *m*
	203 *m*
6	133 M
7	220 M
8	27 *m*
	290 *m*
9	242 M
	340 M
10	168 M
	148 M
	172 *m*
11	60 M
	129 *m*
	351 *m*
13	280 M
	94 *m*
14	310 *m*
15	57 M
	146 *m*
16	47 M
	67 M
	129 M
17	330 M
	119 M
	254 *m*
19	276 M
	272 M
20	28 M
	136 M
	263 *m*
	38 *m*
21	83 M
	64 M
	159 M
22	182 M
	293 *m*
	77 *m*
	115 *m*
23	81 M
	169 *m*
24	5 *m*
25	94 M
26	191 M
	192 *m*
27	281 *m*
28	267 M
	130 *m*
29	337 M
30	232 *m*
31	19 M
	66 M
	207 M
	315 M
	215 *m*
	199 *m*

April.

d	No.
1	327 M
	46 M
2	336 *m*
3	286 *m*
	219 *m*
4	21 *m*
	250 *m*
5	193 *m*
	4 *m*
	240 *m*
6	154 *m*
	206 *m*
7	299 M
	269 M
	338 M
	325 *m*
	279 *m*
8	35 M
9	254 M
	119 *m*
	265 *m*
	39 *m*
11	44 M
	232 M
12	305 M
13	255 M
	223 M
	350 M
	222 *m*
14	184 M
	62 M
	49 *m*
15	298 *m*
16	210 M
17	319 *m*
18	129 *m*
20	121 *m*
	81 *m*
21	143 M
	213 M
	156 *m*
22	119 M
	132 M
	261 M
23	361 M
24	129 M
	41 M
	94 *m*
26	364 *m*
27	72 M
	339 *m*
	170 *m*
28	166 M
	249 M
	360 M
29	328 *m*
	114 *m*
30	307 M
	88 *m*
	71 *m*

May.

1	298 M
2	198 M
	117 M
	131 M

May (*con.*).

d	No.
2	9 M
3	179 M
	254 *m*
4	279 M
	94 M
7	63 M
8	314 M
	81 M
	58 *m*
9	186 M
	232 *m*
	340 *m*
10	228 *m*
11	317 M
12	142 M
13	217 M
	144 *m*
14	351 M
	218 M
	119 *m*
15	108 M
	34 *m*
16	97 *m*
17	43 M
	27 M
19	30 M
	229 M
	183 M
20	335 M
	78 *m*
	8 *m*
21	232 M
	353 M
	32 M
22	154 M
	51 M
	208 M
	56 *m*
	22 *m*
2	300 M
	323 *m*
25	254 M
26	26 M
	59 *m*
27	257 M
	119 M
	137 M
	129 *m*
28	70 *m*
	140 *m*
29	73 *m*
	369 *m*
30	199 M

June.

	250 M
	80 *m*
	255 *m*
2	25 M
	129 M
	293 M
	349 M
	266 *m*
4	94 *m*
	227 *m*
	299 *m*

June (*con.*).

d	No.
5	297 *m*
	81 *m*
6	289 M
	7 *m*
7	5 M
8	176 M
	346 M
	363 M
10	262 M
	175 M
	45 *m*
11	180 M
	105 M
12	320 M
	141 M
	239 *m*
	242 *m*
13	172 M
	53 *m*
14	279 *m*
15	202 M
	292 M
16	94 M
	3 M
17	359 *m*
	232 *m*
	284 *m*
18	31 M
	329 M
	276 *m*
	254 *m*
19	119 *m*
20	38 M
	248 *m*
21	331 M
	182 *m*
	2 *m*
22	75 M
	240 M
	164 *m*
	20 *m*
23	88 M
	81 M
24	258 *m*
	155 *m*
	298 *m*
25	123 *m*
26	330 *m*
27	120 M
28	133 *m*
29	232 M
	29 M
	19 *m*
30	328 M

July.

1	50 *m*
2	119 M
	199 *m*
3	169 M
4	336 M
5	49 M
	358 *m*
	129 *m*
	154 *m*

July (*con.*).

d	No.
6	286 M
	146 M
	216 *m*
	184 *m*
7	121 M
	282 *m*
	148 *m*
8	198 *m*
	118 *m*
	55 *m*
10	300 M
	298 M
	129 M
	279 M
	87 *m*
1	254 M
	366 M
	255 M
	152 *m*
14	319 M
	39 M
	311 M
	307 *m*
15	158 M
	94 *m*
17	18 M
18	78 M
	340 M
	10 *m*
2	144 M
	348 M
	302 *m*
	81 *m*
22	153 M
	325 M
	113 *m*
2	249 M
24	209 M
	301 *m*
	119 *m*
	362 *m*
26	100 M
	232 *m*
2	94 M
	206 M
	337 *m*
2	190 M
	89 M
29	203 M
	365 M
	86 M
	134 M
	131 *m*
	157 *m*
30	77 M
	222 M
	341 M
	351 *m*
31	193 M
	130 M
	269 *m*

August.

1	211 M
	273 M
	220 *m*

August (*con.*).

d	No.
2	317 *m*
	333 *m*
3	215 M
	107 M
	333 M
	369 M
	103 M
	23 *m*
	179 *m*
	254 *m*
4	316 M
	35 *m*
	309 *m*
5	290 M
	17 *m*
	65 *m*
6	119 M
	246 M
7	232 M
8	339 M
	81 M
9	24 M
	6 *m*
10	347 M
11	11 M
13	8 M
	129 *m*
15	299 M
	224 M
	293 *m*
16	168 *m*
18	326 M
	129 M
20	154 M
	279 *m*
22	27 *m*
	125 *m*
24	192 M
	233 M
	123 M
25	265 M
	151 M
	33 *m*
	94 *m*
26	254 M
27	368 M
	250 *m*
28	170 M
	185 M
	40 M
	88 *m*
29	119 *m*
	255 *m*
	327 *m*
30	228 M
	83 *m*
31	21 M
	199 M
	297 M

September.

1	188 M
	67 *m*
2	334 *m*
3	97 M

Septemb (*con.*).

d	No.
3	172 *m*
	298 *m*
	232 *m*
	280 *m*
4	278 M
	56 M
	25 *m*
5	76 M
	81 *m*
6	94 M
	72 *m*
7	13 *m*
	237 *m*
8	115 M
9	30 *m*
10	219 M
	338 *m*
1	119 M
	189 M
	16 M
12	328 *m*
14	274 M
	312 M
	96 *m*
	38 *m*
15	232 M
16	279 M
	307 M
	121 *m*
	350 *m*
17	240 *m*
	143 *m*
18	9 *m*
	353 *m*
	340 *m*
19	298 M
	269 M
	254 *m*
20	19 M
	129 *m*
21	266 M
	276 M
	223 *m*
22	142 *m*
23	81 M
24	207 M
	184 M
25	244 M
	259 M
	242 M
26	129 M
	133 M
27	114 M
	71 M
	191 *m*
28	62 *m*
29	200 M
	239 M
30	93 M
	130 *m*

October.

1	141 *m*
	49 *m*
2	351

Maxima and Minima of Variable Stars (continued).

October (con.).		October (con.).		October (con.).		October (con.).		November (con.).		November (con.).		November (con.)		December (con.).		December (con.).	
	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.
2	199 *m*	3	203 *m*	23	327 M	30	156 M	10	122 M	20	1 M	28	286 *m*	7	129 *m*	18	19 *m*
3	148 M	14	361 *m*		323 M	31	27 M		104 M		232 *m*	29	118 M		8 *m*	19	287 M
	147 M	16	34 M		317 M		48 M		59 M	2	119 M		2 M	9	258 M	20	37 M
	119 *m*		119 M		336 *m*				179 M		294 M	30	307 *m*	10	20 M	21	254 *m*
	154 *m*		295 M	24	250 M	November.			342 *m*		160 M				198 *m*	22	49 M
4	198 M	7	249 M		232 M		371 M		298 *m*		168 M	December.			106 *m*	23	337 M
5	300 *m*		131 M		146 *m*		132 *m*		263 M	22	279 M		301 M	11	213 M		299 M
6	70 M		94 M		289 *m*		183 *m*		328 M		3 *m*		199 M		38 M		33 M
	144 *m*	19	332 M		229 *m*	2	169 *m*	14	87 M	2	32 *m*	2	364 M	12	129 M		63 *m*
	94 *m*		318 M	2	216 M	4	129 M		31 *m*	24	84 M		45 M		262 *m*	24	81 M
7	212 M		73 M		164 M		284 M	6	227 M	2	113 M		232 M	13	171 M	25	370 M
	78 *m*		363 *m*		10 M		357 M		47 *m*		314 *m*		121 M		144 M	26	119 M
8	255 M	20	186 *m*		15 M		254 *m*		94 *m*		193 *m*	4	240 M		358 M	27	88 *m*
	360 *m*	2	330 M		293 M	5	225 M		206 *m*	26	315 M		282 M		119 *m*		94 *m*
9	359 M		88 M		174 M		174 *m*	17	257 *m*		255 *m*		4 M	14	24 *m*	28	123 *m*
11	80 M		218 *m*	27	356 M	6	344 M	18	264 M	27	254 M		25 M		210 *m*	29	242 *m*
	53 M		175 *m*		279 *m*		319 *m*		58 M		195 M		78 M	15	220 M		232 *m*
	254 M		81 *m*		369 *m*	7	39 *m*		154 M		335 M	6	81 *m*	16	142 M	30	201 M
12	299 *m*	22	155 M	28	60 *m*	8	81 M		43 *m*		340 M		172 M		228 *m*		297 *m*
	232 *m*		313 M	29	7 M		119 *m*		5 *m*		209 *m*		248 M		310 *m*	31	46 M
13	140 M		12 M		182 M	9	338 M	19	339 *m*	28	94 M		52 M	17	184 *m*		202 M
	66 *m*		181 *m*		129 *m*		222 *m*	20	329 M		298 M		125 M	18	161 *m*		265 *n*
															351 *m*		26 *n*

SPECTROSCOPIC DOUBLE STARS.

No.	Star.	R.A.	Dec.	Discoverer.	No.	Star.	R.A.	Dec.	Discoverer.
		1900.					1900.		
		h m	° ′				h m	° ′	
1.	η Andromedæ .	0 52	+22 52	Campbell.	25.	α Virginis ……	13 20	−10 39	Vogel.
2.	α Ursæ Minoris	1 23	+88 47	,,	26.	α Boötis ……	14 6	+25 34	Wright.
3.	ξ Piscium ……	1 48	+ 2 42	,,	27.	β Lupi ………	14 52	−41 43	Fleming.
4	ξ Ceti …………	2 8	− 8 23	,,	28.	ε Libræ ………	15 19	− 9 57	Campbell.
5.	12 Persei ……	2 36	+49 47	,,	29.	θ Draconis ..	16 0	+58 50	,,
6.	τ Persei ………	2 47	+52 22	,,	30.	β Herculis …	16 26	+21 42	,,
7.	β Persei ………	3 2	+40 34	Vogel.	31.	μ_1 Scorpii ……	16 45	−37 53	Bailey.
8.	α Persei ………	3 17	+49 31	Newall.	32.	λ Draconis …	16 56	+65 18	Campbell.
9.	λ Tauri ………	3 55	+12 12	Belopolsky.	33.	ω Draconis …	17 38	+68 48	,,
10.	α Aurigæ ……	5 9	+45 54	Newall. Campbell.	34.	χ Draconis …	18 24	+72 42	,,
11.	β Orionis ……	5 10	− 8 19	Vogel.	35.	2 Aquilæ ……	18 37	− 9 9	Wright.
12.	δ Orionis ……	5 27	− 0 22	Deslandres.	36.	6 Aquilæ ……	18 42	− 4 52	,,
13.	β Aurigæ ……	5 52	+44 56	Maury.	37.	β Lyræ ………	18 46	+33 15	Pickering.
14.	V Puppis ……	6 36	−48 9	Pickering.	38.	113 Herculis …	18 51	+22 31	Wright.
15.	ζ Geminorum. .	6 58	+20 43	Belopolsky.	39.	υ Sagittarii …	19 16	−16 10	Campbell.
16.	α_1 Geminorum .	7 28	+32 6	,,	40.	η Aquilæ ……	19 47	+ 0 45	Belopolsky.
17.	ε Hydræ ……	8 42	+ 6 48	Campbell.	41.	31 Cygni ……	20 10	+46 26	Campbell.
18.	θ Ursæ Majoris	9 26	+52 8	Belopolsky.	42.	β Capricorni …	20 15	−15 6	,,
19.	ο Leonis ………	9 36	+10 21	Campbell.	43.	κ Pegasi ……	21 40	+25 11	,,
20.	ε Leonis ………	9 40	+24 14	Adams.	44.	ι Pegasi ………	22 2	+24 52	,,
21.	ξ Ursæ Majoris	11 13	+32 6	Wright.	45.	δ Cephei ……	22 26	+57 54	Belopolsky.
22.	93 Leonis ……	11 43	+20 46	Campbell.	46.	η Pegasi ……	22 38	+29 42	Campbell.
23.	ε Ursæ Majoris	12 50	+56 30	,,	47.	π Cephei ……	23 5	+74 51	,,
24.	ζ Ursæ Majoris	13 20	+55 27	Pickering.	48.	λ Andromedæ .	23 33	+45 55	,,

The period of these stars, so far as they are yet known, are as follows:—(2) 3·95 days, (9) 3·91 days (10) 104 days, (12) 1·92 days, (14) 3·114 days, (15) 5·375 days, (16) 2·93 days, (18) 5–7 days, (19) 14·5 *days, (25) 4·013 days, (28) several* months, (31) 1·46 days, (34) 9⅓ months, (40) 7·176 days, (43) 6 days *45) 5·375 days, (46) 818 days, (48) 19·2* days.

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observer.
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½ ; 7½	345·0	1·46	11·75	B.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	147·1	0·42	11·82	Ls.
3.	β 395	0 32·2	−25 19	6·5 ; 6·7	286·7	0·51	9·72	Ait.
4.	β 495	0 43·4	+18 9	8 ; 8	224·4	0·48	11·80	W. B.
5.	β 866	0 39·1	+42 34	9 ; 9	69·1	1·70	11·75	B.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½ ; 7½	220·2	5·24	11·09	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	315·0	0·43	11·84	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	21·6	1·10	11·04	M.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½ ; 7	352·4	1·56	11·83	B.
10.	β 1000	1 30·3	−30 26	8 ; 12	8·8	1·40	8·69	β.
11.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·6	1·42	10·05	Ls.
12.	*p* Eridani	1 35·7	−56 42	6 ; 6	223·0	8·12	11·05	T.
13.	Σ 205, γ Androm. BC.	1 57·8	+41 51	5½ ; 6	114·9	0·40	10·17	Ls.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	62·2	9·76	10·08	Ls.
15.	Σ 228	2 7·3	+56 59	6½ ; 7½	94·2	0·53	10·29	Ls.
16.	Σ 305	2 41·8	+18 58	7½ ; 8	313·3	2·91	11·04	W. B.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½ ; 6	200·3	1·55	11·86	Ls.
18.	β 525	2 53·5	+21 13	7½ ; 7½	141·9	0·30	11·43	Ls.
19.	β 1030	3 4·1	+21 20	8½ ; 8½	155·7	0·53	10·99	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½ ; 7	13·8	0·17	10·10	Ls.
21.	Σ 425	3 33·8	+33 49	7½ ; 8	268·3	2·37	11·02	W. B.
22.	OΣ 531	4 0·8	+37 49	6½ ; 8½	129·8	1·99	11·11	Ls.
23.	Σ 518, o^2 Eridani	4 10·6	− 7 49	8½ ; 10½	73·6	2·39	9·11	Ait.
24.	β 883, AB	4 45·6	+10 54	7½ ; 8	60·4	0·26	10·15	Ls.
25.	,, AC	……	……	7½ ; 10	156·0	18·10	11·09	Ls.
26.	Σ 535	4 17·7	+11 9	7 ; 8	318·1	1·77	11·08	W. B.
27.	Aldebaran	4 30·2	+16 18	1 ; 13½	109·3	31·04	10·42	Ls.
28.	OΣ 98, *i* Orionis	5 2·4	+ 8 23	6 ; 6½	170·4	0·73	10·21	Ls.
29.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·6	3·33	10·20	M.
30.	Σ 749	5 30·9	+26 53	6½ ; 7½	160·8	0·72	11·13	W. B.
31.	Σ 932	6 28·5	+14 49	8 ; 8	324·6	1·94	10·10	Ls.
32.	OΣ 149	6 30·0	+27 22	7 ; 9	280·0	0·72	11·22	Ls.
33.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	116·7	1·39	10·31	Ls.
34.	Sirius	6 40·6	−16 34	1 ; 10	135·0	5·13	11·25	Ait.
35.	OΣ 156	6 41·6	+28 18	6½ ; 7	305·9	0·49	10·14	Ls.
36.	Σ 1037	7 6·6	+27 24	7 ; 7	302·5	0·77	10·30	Ls.
37.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	110·7	1·56	10·84	B.
38.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	211·0	7·44	11·23	W. B.
39.	β 579	7 27·9	+33 20	8 ; 10½	218·7	0·95	11·32	Ls.
40.	OΣ 175	7 28·7	+31 11	6 ; 6½	329·0	0·71	10·19	Ls.
41.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	226·4	5·95	10·19	Ls.
42.	Procyon	7 34·0	+ 5 29	1 ; 8½	338·5	5·13	11·20	Ait.
43.	Σ 1187	8 3·2	+32 31	7 ; 8	243·1	2·37	11·26	W. B.
44.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	1·9	1·05	11·24	W. B.
45.	,, ,, AC	……	……	5½ ; 6½	114·8	5·48	11·24	W. B.
46.	,, ,, BC	……	……	7½ ; 6½	127·1	6·11	11·24	W. B.
47.	β 205	8 28·8	−24 16	7 ; 7	236·5	0·75	8·25	Ait.
48.	β 208	8 34·6	−22 20	7 ; 8	76·2	0·45	8·29	Ait.
49.	Σ 1273, ε Hydræ AB	……	……	4 ; 6	276·0	0·13	11·27	Ls.
50.	Σ 1273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	232·2	3·51	11·27	Ls.
51.	Σ 3121	9 11·9	+29 0	7½ ; 7½	20·6	0·34	11·29	Ls.
52.	OΣ 201	9 18·0	+28 20	7 ; 11	220·1	1·31	10·30	Ls.
53.	Σ 1348	9 19·2	+ 6 50	7½ ; 7½	319·9	1·80	10·31	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	111·6	0·64	11·30	B.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	313·0	2·15	11·31	Ls.
56.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	91·2	0·48	8·91	Ait.
57.	OΣ 215, P. x. 23	10 10·9	+18 14	6½ ; 7	209·2	0·87	10·24	M.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	118·3	3·67	10·24	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	75·5	0·82	10·31	B.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	296·5	0·39	11·32	B.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Observer.
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	318·4	0·85	11·26	B.
62.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	147·5	2·29	11·32	Ls.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	53·2	2·17	10·43	M.
64.	OΣ 235	11 26·7	+61 38	5½ ; 7	124·6	0·48	8·50	Ls.
65.	β 603	11 53·5	+14 50	6½ ; 10	315·6	0·84	10·44	Ls.
66.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½ ; 8	220·1	1·26	11·39	Ls.
67.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	150·4	6·00	11·41	T.
68.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	83·1	1·07	11·30	Ls.
69.	β 112	12 55·2	+19 1	9 ; 9½	294·6	2·03	10·43	Ls.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	339·6	1·92	11·40	Ls.
71.	β 933	13 30·0	+33 39	8 ; 8½	29·5	2·32	11·37	Ait.
72.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	134·2	0·94	10·43	Ls.
73.	Σ 1785	13 44·5	+27 29	7½ ; 8	280·1	1·40	11·38	Ls.
74.	OΣ 278	14 8·3	+44 39	7½ ; 7½	90·2	0·40	11·50	Ls.
75.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	179·7	1·45	10·47	M.
76.	α Centauri	14 32·8	−60 25	1 ; 1	210·7	22·01	11·33	T.
77.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	150·9	0·31	11·34	Ls.
78.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	328·4	2·65	10·53	M.
79.	Σ 1879	14 41·3	+10 4	8 ; 8½	132·7	0·46	11·48	Ls.
80.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	197·3	2·65	11·49	M.
81.	β 31	14 47·8	+19 9	8½ ; 9½	197·4	1·39	11·34	Ls.
82.	OΣ 287	14 47·9	+45 20	7 ; 8	321·7	0·70	11·52	B.
83.	β 239	14 52·7	−27 15	5½ ; 6	316·5	0·99	8·28	Ait.
84.	Σ 1932, ι Cor. Bor.	15 14·0	+27 13	6½ ; 7	150·8	0·84	10·55	M.
85.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	11·0	0·87	11·62	M.
86.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½ ; 7½	72·1	0·00	11·72	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	181·4	0·94	11·52	Ls.
88.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	110·6	0·54	11·42	Ls.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	142·9	0·84	10·57	M.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	215·3	0·94	6·46	M.
91.	Σ 2032, σ Coronæ	16 11·1	+34 7	6 ; 6½	210·3	4·38	10·72	Ls.
92.	Σ 2055, λ Ophiuchi	16 25·8	+ 2 12	4½ ; 5½	53·2	1·20	10·53	M.
93.	Σ 2084, ζ Herculis	16 37·5	+31 47	3 ; 6½	222·6	1·07	11·70	Ls.
94.	Σ 2114	16 56·5	+ 8 36	6½ ; 8	160·1	1·26	11·51	B.
95.	Σ 2140, α Herculis	17 10·1	+14 30	2½ ; 6	112·1	4·88	11·52	W.B.
96.	β 629	17 15·0	+32 10	8½ ; 8½	344·1	1·15	11·43	Ls.
97.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	327·7	0·94	11·63	B.
98.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	256·9	1·76	10·54	M.
99.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	226·1	1·58	11·63	W.B.
100.	OΣ 358	18 31·3	+16 54	6½ ; 7	193·0	1·87	11·54	W.B.
101.	β 648	18 54·2	+23 39	6 ; 9	220·0	1·11	11·61	Ls.
102.	γ Coronæ Aust.	19 0·0	−37 12	5½ ; 5½	143·3	1·93	11·40	T.
103.	Σ 2525	19 22·9	+27 8	7½ ; 7½	320·8	0·53	11·70	Ls.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	297·7	1·69	11·80	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	80·6	1·11	11·60	Ls.
106.	β 151, β Delphini	20 32·7	+14 15	3½ ; 5½	17·5	0·50	11·75	Ls.
107.	Σ 2744	20 57·8	+ 1 9	6½ ; 7½	163·0	1·33	11·72	Ls.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	118·0	1·47	11·84	W.B.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	122·1	2·60	11·86	Ls.
110.	Σ 2878	22 9·6	+ 7 29	6½ ; 9	124·2	1·11	11·81	B.
111.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	222·2	0·75	11·84	W.B.
112.	β 79	23 12·5	− 2 4	8 ; 8½	87·1	1·03	11·78	B.
113.	β 80	23 13·2	+ 4 53	8 ; 8½	343·1	0·62	8·78	Ait.
114.	β 720	23 29·0	+30 46	6 ; 6	163·8	0·30	11·37	Ls.
115.	Σ 3050	23 54·6	+33 10	6 ; 6½	214·4	2·55	11·88	W.B.

In the column headed "Observer," the following abbreviations are used:—

L.	Leavenworth.	M.	Maw.	B.	Bryant.
Ls.	Lewis.	Ait.	Aitken.	Bd.	Barnard.
β.	Burnham.	T.	Tebbutt.	W.B.	Bowyer.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 December 13.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. 80 presents have been received since the last Meeting; of these I should like especially to call attention to Prof. Newcomb's new book called "The Stars: a Study of the Universe," presented by the Author.

A vote of thanks was passed to the donors.

The President. I would take this opportunity to announce to the Fellows that the Appendix to Volume LIV. of the Memoirs of the Society is now published. It contains the account of the observations of the Total Eclipse of the Sun, in May 1898, made by Sir Norman Lockyer, Capt. Chisholm Batten, and Prof. Pedley. This book will be given to any Fellow of the Society who applies to Mr. Wesley for it.

The President. We have received a very interesting paper from America, which I will ask the Secretary to read.

Mr. Dyson then read the paper by Mr. S. C. Chandler, being a "Contribution to the History of the Reflex Zenith-Tube." This paper showed how a correction to the assumed value of the constant of aberration, the parallax of γ Draconis, and the amplitude and phase of the annual term of the variation of latitude introduced four unknown quantities, whereas the annual term in the observed zenith-distance of γ Draconis is only capable of giving two quantities. When the annual term of the variation of latitude is omitted, an erroneous value of the aberration and a negative of parallax of γ Draconis is found. These anomalies are, however, explained by variation of latitude, and in this connection the observations with the reflex zenith-tube are of great value. A slide was shown to illustrate the main features of the instrument.

The Astronomer Royal. I wish, first, to express my admiration of the persistency with which Mr. Chandler has worked at these residual discordances, and from them has deduced this very small motion of the pole of the Earth, and has established it upon such a secure basis that he is able to explain the anomalies which have puzzled us so long in connection with this reflex zenith-tube. I feel that I ought to congratulate him heartily upon his success. This has been, as he has said, one of the most disappointing instruments with which we have had to deal. I was very much disappointed at the results which were deduced from it, and in 1882 made an effort to get rid of any possible source of systematic error. The only source of systematic error I thought of was that there might be some effect of temperature on the micrometer, which was made of gun-metal, and we made a long series of experiments by taking transits over the wires. We had a focussing-rod made of the same material, which was used to get the distance from the object-glass to the surface of the mercury, and a large number of observations were taken in the years 1883 to 1886. Prof. Turner, who came to Greenwich soon after the series began, took great interest in the matter, and the observations were discussed, but no satisfactory conclusion could be come to. We felt the result to be disappointing, but now I think it is everything we could wish for, because Mr. Chandler has given an explanation of the discordances which was never dreamed of at that time. This long series of determinations will, I hope, prove very useful in discussing the observations made with this instrument. We have not by any means given up observing with the reflex zenith-tube since 1886. From 1883 to 1886, 141 transits were observed for the determination of the effect of temperature and of focus-reading on the observations; and after that date regular observations of zenith-distance were resumed, and special efforts were made to secure observations in the daytime, but with small success. That finally disheartened us, and when at the end of 1899 we found the wires had been swept away, the observations were discontinued. At that time we were engaged with the Ten-Year Catalogue, and I did not wish to run the risk of having a gap at 18 hours R.A., which would have resulted if we had gone on with the observations. As I have said, I was disheartened at the result, but now that Mr. Chandler has shown that we have reason to hope for better success we shall resume them. Since 1886 we have 300 unpublished observations, and these observations, as well as the discussions made of scale-value from 1883 to 1886, are quite ripe for publication, and I hope we shall soon publish them. There is one point in Mr. Chandler's paper, and in which I concur with him, namely, that some improvements might be made in the observations so as to get rid of accidental error. After our experience of the amalgamated mercury-trough used by the French observers for the Paris longitude determination, a similar trough was substituted in 1889 for the trough with india-rubber suspension in use with the reflex zenith-tube. The substitution of this trough has certainly effected an improvement, and I think a further improvement might be effected by having a special observer, who is familiar with the instrument, assigned to make the observations. I am afraid we shall never get over the difficulty of securing observations of γ Draconis in the winter months in the daytime, but that is more a matter of climate than anything else. The special object for which the instrument was first devised was for the determination of the constant of aberration, but unfortunately about six o'clock in the morning, when the effect of aberration is at a maximum, we get very few observations indeed, and the observations are practically confined to one-half of the year. There are also a certain number of other stars which pass near enough to the zenith to be observed with the reflex zenith-tube, and two of those stars have been observed on a few occasions, and I hope that now we are encouraged to go on with the work we shall get more observations of those stars. We at Greenwich are very much indebted to Mr. Chandler for having rehabilitated this instrument.

Prof. R. A. Sampson (giving a description of Adams's MSS. on the perturbations of Uranus). The book which I have brought to show the Society this evening contains very nearly all the MSS. which Adams ever wrote on the perturbations of Uranus. They lay very much as he left them for 50 or 60 years, and then they were entrusted to me by Mrs. Adams to put into order and preface by some sort of catalogue, and were presented by her to St. John's College, Cambridge, where Adams was an undergraduate and a don during the time he thought about Uranus at all. No apology is needed for bringing this matter before the Society, because the interest in it will remain for a long time to come. It may be necessary, perhaps, to give a very short summary of the position of the problem as it presented itself to Adams. Uranus, after its discovery in 1781, was found to have been observed by a number of persons accidentally. It had been observed by Flamsteed, Mayer, Bradley, and Lemonnier: and Bouvard, in constructing his Tables, attempted to unite these observations with others; he found that such a procedure threw intolerable errors upon the new observations, and he had to choose between the old and the new, and he sacrificed the old entirely. It was soon apparent that future observations would be not less discordant. To explain this, various hypotheses were current; to many minds the hypothesis was that it was an exterior planet. Sir Geo. Airy records that he was written to by Mr. Hussey as to the feasibility of such an explanation, but Mr. Hussey did not feel able to take it up. Later on, when the anomalies became more glaring, Bessel wrote to some English astronomer that he had in mind that the perturbations were due to some exterior planet. The same hypothesis was discussed by M. Eugène Bouvard with Sir Geo. Airy, but Sir Geo. Airy was sceptical with regard to it. He thought several revolutions would be necessary before the matter could be verified.

This was the position at which the problem stood when Adams formed the resolution to investigate, "as soon as possible after taking his degree," these irregularities, and characteristically wrote this resolution down upon a scrap of paper, and this precious memorandum is the first page of this volume. Adams was prevented from attacking it for two years, but he set about it in 1843 by collecting observations. The early observations were given by Bouvard, and from 1821 onwards they had to be collected from the *Ast. Nach.* and other sources. In 1843 his observations were collected and reduced, and he set about making his first solution and carried it out. This solution may be considered the most interesting part of the volume. Adams said: "In 1843 I attempted the first solution of the problem.... and found that a good general agreement between theory and observation might be obtained."

Fortunately, this early solution was complete and intelligible, though it was most difficult to follow. For it was written in part on the backs and in unoccupied corners of sheets devoted to other work, as though his eagerness to finish would not wait while he got a few more sheets of paper. And, curiously, at the very point of writing down the planet's place, Adams, who carried through fabulous computations without error, makes a mistake and puts down the wrong place. This mistake was corrected in the MS. probably as soon as made, and the result of his first solution was a very good approximation to the position of the planet. If reduced to epoch 1846, it gives the longitude of the planet 309°·1. It was actually then 327°·0, and therefore was a very good first approximation to the position of the planet, and the actual residuals were hardly inferior to those which he finally obtained. This solution was made in September 1843. Coming to another part of the subject, I may say that in reviewing a large problem like this one is too apt to come to the conclusion at once. There is a large mass of preliminary calculation before the real question can be so much as asked. In the case of Neptune there was a full share of these difficulties, and where Leverrier had a merit greater than that of Adams, it was that he exhausted all that could be said in the way of preliminary discussion of faulty determination of orbit, badly computed perturbations, and so forth. Adams did in this work what was necessary, but no more. What was necessary was to clear out some inequalities which had been discovered since Bouvard's time. Besides that, there was the possibility that Bouvard's elliptical orbit of Uranus had been inaccurately determined and that he had made some mistakes in the perturbations. Adams at first trusted to the verification of Bouvard's inequalities which was furnished by the independent calculation of Pontécoulant's *Système du Monde*, but he finally checked these himself. From first to last Adams made no fewer than six separate solutions of the problem.

These solutions gave for the heliocentric longitude of the planet in 1846 October:—

I.	1843, Sept. 30	309°·1
II.	? 1844	319 ·4
III.	1845, April 28	330 ·2
IV.	1845, Sept. 18	326 ·5
V.	1845, October	328 ·0
VI.	1846, August	328 ·5

At the same time the actual position of the planet was 327°·0.

There are some questions that arise regarding the prediction of Neptune's place to which some time might well be devoted in the proper place. Firstly, seeing that the orbits of Adams and Leverrier both differed widely from the truth in all except the essential point of the planet's position, how far was either a real prediction at all? Next, seeing that both agree in this one essential, how far was the remaining difficult discussion a necessity? Thirdly, was it any real test of the validity of this theory, which was based on observations of Uranus at opposition, to require it to explain errors in radius vector which had been observed at quadratures?

The President. I think that the Society and the astronomical world in general should be grateful to Prof. Sampson for the work that he now brings before us. I myself have always felt that the full and true history of the circumstances relating to the discovery of Neptune would some day be brought to light, but I did not expect this would happen until at least a century after the facts themselves, and here we have the details put before us in this lucid way in only about half that time. The three questions Prof. Sampson asks are intensely interesting, and I hope that when his paper is printed we shall see that he has not only asked them, but has given replies.

Prof. Turner. I should like to say a few words of admiration for the work which Prof. Sampson has done, and to congratulate him upon having done it, because I happen to have seen something of the labour that was required. It has been cheerfully undertaken by Prof. Sampson, although it has taken up a great deal of time which he could well have spent on important work of his own, and, as the President has said, it has been completed at a time much earlier than could have been expected, and here is this volume before us within a few years of Adams' death. One thing more I should like to say. It is difficult to realize that this volume now before us, which has been presented to St. John's College, Cambridge, represents one of the closing scenes of a drama which rent the astronomical world in twain 55 years ago, and I think that some mark of appreciation of this fact on the part of this Society would be fitting at the present moment. I have had the privilege of seeing some of the papers, and I shall venture to propose to the Council at their next meeting that the few pages in that volume representing the "solution of 1843" be published by the Society in facsimile (if leave can be obtained from St. John's College), because I think that these results, completed two years before those of which any announcement was made, should be before the world; and I cannot help thinking that to reproduce the MS. of Adams in his beautiful handwriting would be an excellent memento. (Applause.)

The President. I should like to ask Prof. Sampson whether he thinks the solution of 1843 can be isolated from the rest of the work so as to be intelligible in itself, and what value he attaches to Prof. Peirce's solution, and whether he has ever raised any objection to it himself?

Prof. Sampson. As to the solution made in 1843 it is in a very tangled state, but one or two parts of it—possibly all the significant parts—can be reproduced in facsimile, and I am sure they would be interesting. The actual calculation in which he first determined the position of the planet I think would be very interesting to reproduce. Unfortunately the date is not on that sheet—it is a little further on. It is unquestionably associated with it, but if you wanted the date you would have to reproduce another sheet as well.

The President. Did he use a circular orbit in 1844?

Prof. Sampson. Yes: he used a circular orbit.

The President. I understand he never stated the date of that solution?

Prof. Sampson. He never stated the date. With regard to the other point—my three questions—I did not intend to put them down merely as apples of discord before the Society, and I hope to be able to give an answer to them. Though I do not wish to go into them at present, I think my mind is pretty well made up on the different points; but I have not the books with me, and I wish to take away two or three books to enable me to go into the matter. As to Peirce's criticisms, I must confess I do not know them; but if the President will kindly give me the reference, I shall be glad to read them.

The President. In offering the formal thanks of the Meeting to Prof. Sampson, I should like to mention that this is not the only one of Prof. Adams' researches that has passed through his hands. Many of those present no doubt have seen Adams' work on Jupiter's Satellites, which was set in order and published some time since by Prof. Sampson. The debt that astronomers owe to Prof. Sampson for the ungrudging and long-continued labours which he has so freely bestowed on a most difficult task cannot be over-estimated.

Mr. Hinks read a paper on the accuracy of measures on photographs—remarks on recent papers by M. Loewy and Mr. H. C. Plummer. He said that the point he had to bring before the Society that evening was extremely technical and more than usually dull; it was concerned with a little controversy which had arisen about systematic errors of measures on photographic plates. In a paper published by M. Loewy in the eighth circular of the Conférence Internationale, the systematic error of measurement was treated as if it followed the ordinary law of distribution of accidental error; in fact, M. Loewy maintained that "systematic error is accidental error of another kind." This method of treatment would be wrong if it could be shown that there is a tendency for a compensation of errors to take place when the plate is reversed. M. Loewy and Mr. Plummer have derived from the same material contradictory conclusions—the former that there is not, the latter that there is a sensible compensation. These results are obtained by calculating probable errors from the residuals. As the legitimacy of applying the method of least squares here is in question, it seemed worth while to examine the point by a way that did not involve this method. The result is to show that there is a very marked compensation of error when the plate is reversed. The conclusion is that M. Loewy's method of treating the subject is invalid, and that Mr. Plummer's contention is correct, though the author does not find it possible to agree with him in accepting the truth of M. Loewy's fundamental equation.

Mr. Hollis. We have at Greenwich a large accumulation of information on this subject, deduced from the measures of the Astrographic Catalogue plates, which have been discussed although no formal paper has been prepared for publication. The plates are measured in two positions, being rotated through 180°, and what at first sight might seem to be accidental error proved on analysis to be largely systematic errors, which changed sign on reversal of the plate, thus supporting Mr. Hinks's conclusion. This systematic error varies for different measurers, and so may be considered as a personality. We notice this fact in everyday work, and the change of sign of the personality with reversal gives considerable confidence in the accuracy of the mean result.

Prof. Turner then gave a description of a paper on "A Simple Method of Photographic Surveying with an ordinary Camera." He said it was not exactly an astronomical paper, but the method he wished to put before the Society arose directly out of the astronomical work which they had been doing—that was to say, the measurement of star photographs. He had on several occasions brought before the Society the advantages of using rectilinear methods of measurements and reduction instead of angular. So far as he knew, all attempts to use the camera for survey had started with the theodolite. Now, the theodolite was used for measuring angles, and the essential part of his suggested method was that angles were to be given up. He would show by slides what he proposed by the use of the instrument. Taking a set of rectangular axes at the lens centre, the axis of z being perpendicular to the plate and those of x and y parallel to the plate, in the horizontal and vertical directions: then the co-ordinates of any object photographed being (X, Y, Z), those of its image on the plate are $(-x, -y, -f)$, where f is the distance from lens to plate; and all we can get from a single photograph by measuring the co-ordinate x is the ratio X/Z, which is equal to the ratio x/f. We cannot find either X or Z without another photograph. Now this should be taken at another station, for which either Z or X remained the same: which suggests two methods. First the parallactic or stereoscopic method. Move the camera 100 feet to the right and take a second picture. Then Z will be the same as before, but X will be diminished by 100 feet. Hence the equation $Z = 100f/(x_1 - x_2)$, where x_1 and x_2 represent the measures on the two plates. The co-ordinates X and Y are deduced by equally simple formulæ. No trigonometry is required, only subtraction and division. In the second method the camera is *advanced* 100 feet towards the view; so that X and Y remain the same, but Z is altered by 100 feet. This method had the advantage that it gave the one vital adjustment by the pictures themselves—the one vital adjustment was that the camera must be kept parallel to itself, and they could see from the pictures at once if there was any displacement whatever of the centre of the field of view. In the case of the stereoscopic method, that error could not be determined from the photographs themselves. Therefore he proposed that there should be two cameras strapped one above another, and that both pictures should be taken and then that the cameras should be moved 100 feet, and then that another pair of pictures should be taken. The general method of survey would be to advance in one general direction and at intervals to take these pairs of pictures at distances of 100 feet apart, which would give the general indication of the country. The one thing essential was to have réseaux on the plates, but that was precisely similar in all astronomical work. The method of measuring would be exactly the same as in plates of stars.

Prof. Alex. Herschel. I can sympathize with Professor Turner's high regard for the simplicities of the methods which he has taken such good pains to explain the use of in this paper, from having already known and practised the first of the two processes described, for nearly forty years, under mathematically similar, although practically different, conditions, in finding real heights of meteors on the British Association's Meteor-mapping Charts of 1864–5 *. Those gnomonic or plane-perspective star-charts are in effect equivalent to photographs of the sky taken in a camera with a lens of four inches focal length, pointed vertically upwards; and if a meteor-point—when its observed right ascension has first been converted into hour-angle—is plotted upon one of them as it was noted at one station, and again as it was recorded at another station, b miles from the first in a known azimuth direction, the displacement of the pair of points one from the other, d inches, on the map, if they were well recorded, will be parallel to that direction, and signifies the same as $x_2 - x_1$, the difference of distances of a distant object's pictures from the centres of the plates, in the description and figure of the first method of proceeding in Professor Turner's paper as designed to give us stereographically the object's distance from either of the cameras in a perpendicular or normal direction to the common plane of their pair of photographic plates. To the measured distance, 100 feet, corresponds the two observers' base-line length, b miles: h miles, the meteor-point's vertical height above the map's horizon-plane, is the equivalent of F, the photographed object's distance in feet from either of the camera's locations in the direction of their lens-axis perpendicular to the field-plane of the two exposed glass plates. The lens's focal length, f, and the prime-radius, 4 inches, of the gnomonic map's projection are also like elements of the two similar geometrical constructions, and the desired results in both cases are thus finally obtained by the same equation with only varied symbols:—for $F = \frac{100 \times f}{x_2 - x_1}$, we have $h = \frac{b \times 4}{d}$. But the meteor-point need not lie on the base, as the distant objects enjoined to be photographed in Professor Turner's first or distance-gauging procedure are all assumed to be situated in the same prime-vertical plane of the two observing stations, or in the same normal plane of the photographing cameras, with the measured base-line; but may be over any geographical point to one side or the other of the base, or of the base's prolongation, and yet the rule will not be altered, —from the same set of similar triangles being still used as before in the geometrical construction,—which always gives, by this simple equation, the meteor's height in miles above the map's horizon from the graphic displacement apart of the two plotted points, in whatever part of the map that plotted length, or dwarfed image, in fact of the base itself, imagined conjured up towards one direction or the other from the real meteor-point may happen to be pictured. The opposite of the graphic dwarfing ratio $d : b$, or the above-used enlarging fraction $\frac{b}{d}$, furnishes us, in fact, also with the real offsets in miles, along and across the two observers' base-line, starting from one of their positions, to the geographical spot over which the meteor was vertical; from the perspective copy on the map, from one observed end of the mapped displacement or virtual base-line's effigy, along and across it to the map's centre, of the real base and offsets, and meteor-height, transported ideally to start (all measured oppositely to their true directions) from the meteor-point itself and end at the map's centre of projection, 4 inches above the middle or zenith-point of the level map-face.

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* An outline of the method's use with the maps of that construction and the expression of its result in the form of the same simple equation as that noted by Professor Turner in his paper, was given with illustrated applications to examples, in a paper on "Calculated Heights of Shooting-stars doubly observed in August 1870," in the Proceedings of the Royal Meteorological Society, vol. v. p. 251, January 1871.

If α, β inches are those map-measured offsets from one observing station's, A's, projected meteor-place to the map's centre, α may be called positive or negative as it is towards or from the other observed meteor-place, or (in its final use) towards or from A's consorted observing station, and β will require to be set off finally from the real geographical base-line towards that azimuth-quarter or horizon side, towards which this offset's foot-point on the map bears from the map's centre, expressed in the ordinary scale of real azimuths belonging properly to the map's projection. For the real values l, k of these offsets in miles we will have

$$l = \alpha \times \frac{b}{d}, \quad \text{and} \quad k = \beta \times \frac{b}{d},$$

just as for the vertical offset, or real height in miles, the value is furnished from the vertical prime-radius of the map, or from the correspondingly directed graphic depiction of the height, measuring 4 inches, by the equation $h = 4 \times \frac{b}{d}$.

Supposing that with a base-line of 60 miles the two observed points of commencement of a shooting-star are found, on map-projection, to be 3 inches apart, parallel to the base-line's direction, then the enlarging ratio $\frac{b}{d}$ was $\frac{60}{3}$, or 20, and the real height in miles was $20 \times 4 = 80$ miles; and the same multiplier, 20, would be applied to the measured offsets in inches along and across the displacement-line of 3 inches, from one observed extremity of it to the map's centre, or to the projected zenith, to find the real offset's measures, or the horizontal co-ordinates in miles of the geographical locality of the commencement referred to the selected observing-station and to the real base direction, in order to lay them down correctly and thus to determine on a local map the geographical position of the point directly over which the shooting-star began its real course *.

Having, as I said, had nearly forty years' experience of the pleasure of thus simply reading off meteors' heights, and more latterly also their geographical courses and real path-lengths from the British Association Chart projections, I can most heartily join with Professor Turner in esteeming very highly his easy rule's good merits; and I can very thoroughly endorse his recommendation of at least one of the two ingenious methods of surveying by photography presented in his paper as very eminently deserving the Society's consideration, by its great practical convenience and simplicity in use, as well as by its very certain reliability and its generally very effective accuracy.

Major Hills. Prof. Turner's method strikes me as a very interesting one, but I am rather sceptical as to whether it will be used in actual survey work. Many difficulties come in, one of which is the immense number of plates which would be necessary; and there is, after all, the fact that the man who takes the photographs has only begun, and he has to make the measures of his photographs, and measures can be just as well made on the ground. I doubt if the method will ever be practically used at all. I think that the accuracy that Prof. Turner claims is rather too high, because you generally do not get anything like a defined point to deal with. One practical difficulty arises from the change of view and consequent change in shape—such as a hill-top, which you may see from one point as a sharp pyramid and from another place as a long edge. I think the method might be useful where the seasons are such that there is only a short period in which work can be done in the field. A great deal of good work was done by the Surveyor-General of Canada in Alaska, but in that case it was important to get through the field-work in a few weeks and then calculate it up afterwards.

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* [Some of the details of this explanation have been supplied by Prof. Herschel subsequent to the Meeting.—Eds.]

Prof. Turner. If the method is to be used for purposes of extreme accuracy, then it will be easy to set up sharply-defined points, such as flagstaffs; but if it is merely to give an idea of the country to travellers who do not want to be delayed along the road, then the accuracy need not be so high. Films can be used instead of plates if there is a réseau in front of them, so that the accumulation of plates need not be great.

Mr. Dyson then read a paper by *Mr. W. F. Denning* on the observed motion and duration of the radiant-point of the Perseids.

On the motion of the *Rev. E. Ledger*, seconded by *Major Kingsley Foster*, Messrs. Gibbs, Hollis, and Levander were appointed Auditors of the Treasurer's Accounts for the past year.

The following papers were announced and partly read:—

W. F. Denning. "The Observed Motion and Duration of the Radiant-point of the Perseids."

J. E. Gore. "Observations of Nova Persei."

Rev. S. J. Johnson. "Apparent Paucity of the Leonid Stream."

S. C. Chandler. "Contribution to the History of the Reflex Zenith-Tube."

A. R. Hinks. "On the Accuracy of Measures on Photographs: Remarks on Recent Papers by M. Loewy and Mr. H. C. Plummer."

R. A. Sampson. "Description of Adams's MSS. on the Perturbations of Uranus."

W. F. Denning. "Note on a Large Fireball."

H. H. Turner. "On a Simple Method of Accurate Surveying with an Ordinary Camera."

Radcliffe Observatory, Oxford. "The Leonids, 1901."

F. W. Henkel. "The Leonids, 1901."
H. F. Newall. "On the Orbit of α Persei."

The following gentleman was elected a Fellow of the Society:—

W. J. Greenstreet, M.A., Marling School, Stroud, Gloucestershire.

The following Candidates were proposed for election as Fellows of the Society:—

W. A. Kibbler, M.B., 3 Manor Road, Stamford Hill, N. (proposed by the Rev. T. E. R. Phillips).

C. F. Sandberg, M.A., Magdalen College School, and 6 Parks Road, Oxford (proposed by H. C. Plummer).

Albert Walter, Royal Alfred Observatory, Mauritius (proposed by E. W. Maunder).

E. F. White, Navigation Dept., Merchant Venturers' Technical College, Bristol (proposed by C. T. Whitmell).

THE BRITISH ASTRONOMICAL ASSOCIATION.

A Conversazione was held at Sion College, in the evening, on Wednesday, November 27th, to celebrate the hundredth meeting of the Association. About two hundred and fifty Members and guests, many of whom had travelled considerable distances, were present on the occasion, and the large hall of the College was taxed to its utmost capacity.

A large number of photographs and astronomical and meteorological instruments were on view. Among the exhibits were a number of photographs taken with the Greenwich 30-inch reflector and the 26-inch refractor, lent by the Astronomer Royal, and negatives of the total eclipse taken at Mauritius, lent by the Royal Astronomical Society. The Association itself contributed a selection of drawings and photographs. Mr. Thorp showed some of his well-known grating replicas and other instruments, including a novel arrangement for enabling the whole disc of the Sun to be examined at once by monochromatic light. A special exhibit was sent over from Munich by C. A. Steinheil Sohne, consisting of prisms, flats, object-glasses, and other instruments; while several London firms sent specimens of their optical and meteorological instruments. Several members of the Queckett Microscopical Club brought and exhibited their microscopes.

In the course of the evening two exceedingly interesting addresses were delivered, in each case illustrated by the optical lantern: the one by Mrs. Isaac Roberts, on "Photographs of Celestial Objects taken at the Paris Observatory and at Starfield Observatory, Crowborough, Sussex"; the other by Mrs. E. Walter Maunder, on "The Recent Eclipse Expedition to Mauritius."

The Blue Hungarian Band played a selection of music during the evening.

THE ROYAL METEOROLOGICAL SOCIETY.

THE monthly Meeting of this Society was held on Wednesday, December 18, at the Institution of Civil Engineers, Westminster, *Mr. W. H. Dines*, President, in the Chair.

The Hon. Rollo Russell read a paper entitled "Further Observations and Conclusions in Relation to Atmospheric Transparency." For a number of years past he made daily observations on the clearness of the atmosphere at Haslemere, Surrey, and in the paper he gave the results of the same. The principal conclusions derived from these observations are:—Haze and fog are commonly caused by the mixture of currents at different temperatures. These currents may be local or general, high or low. Thick haze or fog not dependent on differing currents is rare, but differing currents frequently come into contact without producing haze or fog, and fairly clear weather under opposite currents is not uncommon.

A fog may generally be taken *ipso facto* as evidence of the existence in the neighbourhood of a conflict of currents, and prevalent fog or haze commonly signifies that a different wind exists at a high level from that on the surface or at a slight elevation.

The production of fog or haze by mixing currents depends chiefly on differences in their temperature.

Broadly-extended westerly winds, with westerly upper currents, are the clearest, and visibility may reach the highest figures during their prevalence, whether they are dry or nearly saturated.

Easterly and north winds are the most hazy, owing to the ordinary upper current from the west being seldom displaced by them, and to the mixture of these masses of air of different temperatures. When, as an exception, east and north winds are clear, it may be presumed, without direct evidence, that the upper current coincides with them in direction. In winter, therefore, unusual clearness in these winds often signifies a long spell of frost.

The other papers were:—"Remarkable Phosphorescent Phenomenon observed in the Persian Gulf, April 4th and 9th, 1901," by *Mr. W. S. Hoseason*; and "The Mechanical Principle of Atmospheric Circulation," by *Capt. R. A. Edwin, R.N.*

The Determination of Jupiter's Mass from the Cape Observations.

[Continued from vol. xxiv. p. 452.]

Another point must be mentioned in this connection.

Prof. Sampson objects to my rule of not introducing as unknowns the corrections * to inequalities of which the maximum geocentric effect is smaller than $0''\cdot05$. Of course I never thought of denying "that quantities even smaller can be derived with much certainty from observations well made and well combined." The meaning of such a rule is, of course, this: that inequalities which present themselves to the heliometer as such small quantities cannot be expected to be determined by heliometer observations with greater accuracy than they are already known, either by theoretical derivation from other elements, or from other observations, or combinations of observations, specially made with a view to the determination of these inequalities. This is the case with Jb^2, which has been determined by different investigators from the secular motion of the nodes. A correction to Jb^2 derived from the Cape observations would thus not have represented a real addition to our knowledge.

Of course quantities which could not be supposed to be known with greater accuracy than they could be determined from these observations, such as the equations of the centre and the latitudes, were introduced as unknowns, irrespective of their maximum geocentric effect.

But I return to the main point. In order to derive approximately the p. e. with which a correction to Jb^2 might have been determined, we may suppose for a moment that the observations of the four satellites could have been separated. There are very nearly 100 observations of each satellite. In the mean the coefficient of a correction to the mean distance would be $\frac{1}{2}\sqrt{2}$; the mean distances could thus be determined with a weight of

$$100 \times (\tfrac{1}{2}\sqrt{2})^2 = 7^2,$$

neglecting the (very considerable) reduction of the weights by the influence of the other unknowns. The p. e. of one observation is $\pm 0''\cdot087$, the p. e. of the mean distances would thus certainly exceed $\frac{1}{7} \times 0''\cdot087$, or $0''\cdot012$. If this p. e. is compared with the quantities (β) (see vol. xxiv. p. 452), it appears that an error in Jb^2 of one-fifth of its amount could not have been detected.

From the equation given by Prof. Sampson (*Obs.* 1901 Oct.

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* There seems to be some confusion in Prof. Sampson's paper between the correction to a quantity and the quantity itself. The max. geoc. effect referred to is, of course, that of the *whole* of the inequality; the *correction* is unknown.

p. 378), we find with sufficient accuracy for the p. e. of a correction to Jb^2, which could be derived from one mean distance,

$$\epsilon_{Jb^2} = 3a\epsilon_a.$$

If we suppose the probable errors of the four mean distances to be the same, we will get for the p. e. of the value of Jb^2, resulting from the combination of the four separate determinations,

$$\epsilon_{Jb^2} = \frac{3}{\sqrt{\Sigma \frac{1}{a^2}}}\epsilon_a.$$

Now ϵ_a certainly exceeds ±0''·012 for $\log \Delta = 0·6$ or ±0·00050 expressed in b as unit. ϵ_{Jb^2}, expressed in the same unit, therefore certainly exceeds ±0·0072, or about one-third of the whole quantity Jb^2 *, as I stated before (*Obs.* Sept. p. 344). I think it must now be clear that I was quite justified in saying in my paper that "the quantity J cannot be determined with any weight from these observations."

c. As to the influence of the uncertainty of Jb^2 on the resulting value of M, Prof. Sampson sums up his objections in his concluding sentence: "Why is it" (viz., my method) "even the best when it introduces the quantity Jb^2, which Mr. de Sitter says he cannot determine, and whose neglect involves quantities greater than the correction to M?" If the word "whose" refers to "the quantity Jb^2," I may remark that I did *not* neglect Jb^2, but only a possible correction to its adopted value. A few lines earlier Prof. Sampson actually speaks about "the correction to Jb^2." If, by some peculiarity of the English language with which I am not acquainted, the word "whose" refers to this correction, then the sentence quoted is not true, as a comparison between the quantities (β) and (γ) (see p. 452) will show.

That the influence on the value of M of any acceptable correction to Jb^2 is negligible compared with the probable error of M is evident almost at first sight, as the mass is determined almost exclusively from satellites III. and IV., while an error of Jb^2 can only have a sensible influence on I. and II. But, to remove all doubts, we can easily derive the approximate effect on the value of M of the same *exaggerated* error in J which was supposed above.

If corrections to M are derived from each of the quantities (β) and then the mean is taken, giving weights on the assumption that the four mean distances are determined with the same accuracy, or, in other words, if we take *

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* That this method of finding the p. e., rough as it is, still gives a fair approximation, may be shown by computing in the same way the p. e. of M. We then find that this p. e. must exceed ±0·047, while it actually is ±0·060 (my paper, p. 66).

$$\text{correction to M} = 3\text{M}\frac{\text{sum of corrections }(\beta)}{\text{sum of mean distances}},$$

we then find

$$\Delta\text{M} = \pm 0{\cdot}032^5.$$

The probable error which I found for M was $\pm 0{\cdot}067$.

The effect on the value of M of an error in $\text{J}b^2$ equal to five times the difference between the two most reliable determinations of that quantity is thus only one half of the probable error of M, or about equal to the effect on M of the remaining uncertainty in the determination of the standard distance, or again very little larger than the effect on M of the reduction of the observations to the centre of the Earth. We will almost certainly over-estimate the uncertainty of $\text{J}b^2$ if we assume it to correspond to a *mean* error equal to the extreme error here adopted. The true probable error of M would then become

$$\pm\sqrt{(0{\cdot}067)^2+(0{\cdot}022)^2} = \pm 0{\cdot}070.$$

It thus appears that the uncertainty of $\text{J}b^2$ adds only a negligible quantity to the probable error of the reciprocal of the mass.

The above may be a sufficient reply to Prof. Sampson's chief point of objection. I have shown :—

(*a*) That an error in the quantity $\text{J}b^2$ of one-fifth of its amount is extremely improbable ;

(*b*) That an error of even greater amount could not have been detected by the Cape observations ; and

(*c*) That an error of that amount would not appreciably affect the correction to M.

3. It seems to me, however, that the true origin of the difference between Prof. Sampson and myself is of a more fundamental nature.

Prof. Sampson again says that I should have "taken the obvious course, which was followed by [my] predecessors, of treating the mean distances....as liable to independent correction." In my former reply (*Obs.* Sept. p. 343) I have already pointed out that Bessel, who may be taken to represent my "predecessors," as all the later investigators have used his formulas, did *not* treat the mean distances as liable to independent correction. He derived four values of the mass, simply because he made four independent determinations of it, one from each satellite; he then took the weighted mean of these four values, and derived the corrections to be applied to his measured mean distances in order to make them consistent with this mass, pointing out that these corrections were considerably smaller than the mean errors of the measured distances (Bessel, *Astronomische Abhandlungen*, ii. p. 64). He evidently did not agree with Prof. Sampson that "the four mean distances are the most valuable result to be got from heliometer observations"; he never thought of attaching any importance to the measured values of the individual mean distances except as a means of determining the mass.

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* To show in how far this method, which, of course, only purports to give an idea of the order of magnitude of the effect, not to derive a real correction, is reliable, I may mention that the quantities (*a*) (p. 449), when treated in the same way, would give a correction $\delta\text{M} = +0{\cdot}090$, which agrees fairly well with the result of the rigorous computation given before.

By a somewhat different reasoning I found for the effect of the supposed error in $\text{J}b^2$, $\Delta\text{M} = \pm 0{\cdot}033^5$.

But we are at present very little concerned with what my predecessors did. In the case of the Cape observations, as has been repeatedly pointed out both by Prof. Sampson and myself, the observations of the four satellites are not independent, and it would be an artificial complication of the problem to treat them as if they were. I may remark again that the assumption which underlies the elimination of the mean distances is not that "the correction to Jb^2 is negligible," but that *its effect on the mass* is negligible. That this is the case I have shown above.

I am still convinced that the course which I followed is the only one which could be taken under the circumstances. The ultimate aim of my investigation was, as mentioned in my introduction, to provide data which would, in combination with those derived from previous and more recent observations, furnish the basis of the computation of new tables of the satellites. I do not think I can express the considerations by which I have been guided better than by quoting the following words from Newcomb ('Astronomical Constants,' pp. 173–74):—

> The practical question which has been before the writer in working out the preceding results is: What values of the constants should be used in the tables of the celestial motions of which the results of this discussion are to form the basis? Should we aim simply at getting the best agreement with observations by corrections more or less empirical to the theory? It seems to me very clear that this question should be answered in the negative. No conclusions could be drawn from future comparisons of such tables with observations, except after reducing the tabular results to some consistent theory. The imposition of such a labour upon the future investigator is not to be thought of. Moreover, there is no certainty that the tables which would best represent past observations would also best represent future ones. Our tables must be founded on some perfectly consistent theory, as simple as possible, the elements of which shall be so chosen as best to represent the observations.

It follows that, when there is a theoretical relation between different elements, as is the case with the mean distances, this relation should not be ignored; and consequently the best values of the mean distances that can be derived from the Cape observations are those which are theoretically consistent with the derived value of the mass and the adopted values of the mean motions and of Jb^2. It is true, when better values of these latter elements will have been derived by a combination of observations taken at different epochs, such as is proposed in my last chapter, then probably some small corrections will have to be applied to these mean distances on account of the corrections to the adopted values of Jb^2 and, perhaps, of the mean motions; but, as I have shown, these corrections will almost certainly be smaller than the p. e. with which the mean distances could have been directly determined, and in the meantime, if the individual mean distances are required, the theoretically consistent values will be decidedly nearer to the truth than such values as could have been derived ignoring the theoretical relation between them.

Gröningen, 1901, Oct. 30. W. DE SITTER.

Changes in the Stellar Heavens.

I HAVE made a further comparison of Al-Sufi's star-magnitudes with modern estimates and measures, and the following notes may prove of interest to observers of variable stars. I have taken stars which lie comparatively near each other in the sky, so that there can be no doubt as to their relative brightness. I have followed the order of the constellations as given by Al-Sufi.

Ursa Minor.—Both Ptolemy and Al-Sufi rated α Ursæ Minoris (the Pole Star) as 3rd magnitude, and β 2nd magnitude, but all modern estimates and measures make the two stars almost exactly equal in brightness.

Draco.—Ptolemy, Sufi, Argelander, and Heis all agree in making ψ distinctly brighter than τ, but the Harvard and Oxford photometric measures make them about equal.

Boötes.—Ptolemy and Al-Sufi agree in rating τ and υ Boötis as equal, but modern estimates and measures agree in making υ distinctly the brighter of the two.

Corona Borealis.—Ptolemy and Al-Sufi rated ε and ι of this constellation as equal (4 mag.), but modern measures make ε about one magnitude brighter than ι.

Cygnus.—Al-Sufi rated Flamsteed 39 Cygni as 4–5 magnitude, and 52 5 mag., but modern estimates and measures make 52 a little brighter than 39. It seems probable that some change has taken place in the relative brightness of these stars.

Ophiuchus.—Ptolemy and Al-Sufi agree in rating Flamsteed 66 and 72 as 4th magnitude, but at present 72 is about one magnitude brighter than 66.

Pegasus.—Ptolemy and Al-Sufi rated γ Pegasi and δ (now called α Andromedæ) as 2–3 magnitude, but at present α Andromedæ is nearly one magnitude brighter than γ Pegasi. The latter star has been suspected of variation.

Andromeda.—Both Ptolemy and Al-Sufi agree in rating the stars σ and θ of this constellation as equal, but Heis makes σ one magnitude brighter than θ. Argelander rated σ a little brighter than θ, but the photometric measures at Harvard and Potsdam make θ slightly the brighter of the two. On October 23, 1899, I estimated the relative brightness as $\theta\ 2\ \sigma\ 5\ \rho$, and about the same on September 23, 1901, which agrees well with the Harvard measures, and fairly well with Al-Sufi. Sir William Herschel found σ just perceptibly brighter than θ. Possibly one of these stars (θ, σ, ρ) may be variable to some extent. They should be watched by variable-star observers, as close to ρ is the known variable star R Andromedæ, and variable stars often occur in groups.

Triangulum.—In this constellation the star η (Fl. 7), which lies closely south of δ and γ, was rated 6th magnitude by Al-Sufi, 5 by Argelander, and 6–5 by Heis, δ Trianguli being rated 5–6 by Al-Sufi, 6–5 by Argelander, and 5–6 by Heis. The photometric measures at Harvard, Oxford, and Potsdam agree in making δ the brighter of the two, thus confirming the estimates of Al-Sufi and Heis, but opposed to that of Argelander. On October 27, 1899, I found $\gamma\ 4\ \delta\ 5\ \eta$, which agrees fairly well with the Potsdam measures, viz., γ 4·32, δ 5·10, and η 5·70.

Taurus.—Ptolemy and Al-Sufi agree in rating *s* Tauri (north of ξ Tauri) 4th magnitude, and two magnitudes brighter than *e* (Fl. 30) Tauri (6th mag.), but modern estimates and measures make them nearly equal (about 5th magnitude). One or other of these two stars has probably changed in brightness since Al-Sufi's time. Perhaps one has faded and the other brightened. Al-Sufi rated η Tauri (Alcyone), the brightest of the Pleiades, as 4th magnitude, but it is now of the 3rd magnitude.

Leo.—π and ρ Leonis were rated by Ptolemy and Al-Sufi as equal (4th magnitude); but ρ is now considerably brighter than π.

Scorpio.—Both Ptolemy and Al-Sufi rated α (Antares) of the 2nd magnitude, but it is now nearer 1st magnitude than 2nd, and it may possibly have brightened since Al-Sufi's time.

Pisces.—Al-Sufi rated γ Piscium 4–5 and ι 4 magnitude, but γ is at present about half a magnitude brighter than ι. γ has probably increased in brightness since Al-Sufi's time. Both Ptolemy and Al-Sufi rated ϵ and ζ as 4th magnitude, but at present ζ is about 5th magnitude.

Orion.—Ptolemy and Al-Sufi agree in making δ, ϵ, and ζ—the stars forming "Orion's belt"—all of the 2nd mag., but at present δ is distinctly fainter than the other two. δ has been suspected of variability, but variation has not been satisfactorily proved, at least in recent years.

Lepus.—Both Ptolemy and Al-Sufi agree in rating the stars ι, κ, λ, and ν as 5th magnitude, but at present ν is much the faintest of the four stars which form a small trapezium about 4 degrees south of Rigel (β Orionis).

Centaurus.—The star Flamsteed 1 (*i*) was rated 4th mag. by Al-Sufi, and Fl. 2 (*g*) 5th mag., but they are now almost exactly equal. In this case it seems certain that a change has taken place in the relative brightness of these stars. J. E. Gore.

Anomalous Occultations.

THE following notes, extracted from the records of some 270 occultations observed at Harrow, may be of interest in connection with this subject, which has recently been brought to notice in the pages of the *Observatory*. The instrument with which the observations were made is a 4½-inch refractor by Cooke, generally used with a single achromatic eyepiece of about 85, the field being limited to 10′, half of which, or less, could be cut off at pleasure by a movable shutter. Occasionally a Kellner eyepiece of 75 was used. In those cases where the invisibility or otherwise of the Moon's dark limb has not been recorded I have added her age. None of these notes would have been made had there been any cloud about to interfere with the observation. It is, of course, probable that some of the causes recorded as "not instantaneous" are merely subjective—due to want of sensitiveness of the retina. The zenith-distance of the Moon is given instead of the hour.

B.D. −16°.5617, 7·0.—Disappeared at dark limb, which was visible. 15^{s} before extinction the star was suddenly reduced to about 9¼ mag.; remained so for 10 or 12 seconds, then seemed to brighten a little. 1885 Dec. 10, Z. 73^{c}.

B.D. +0°.70, 8·3.—Disappeared at dark limb. ☾ age 9 days. Not instantaneous. 1885 Dec. 15, Z. 59°.

85 Ceti, 6·3.—Disappeared at dark limb. ☾ age 9½ days. Extinction gradual during ⅓ or ½ second. No sudden reduction of light. 1886 Jan. 14, Z. 41°.

B.D. −18°.5624, 8·0.—Disappeared at dark limb, which was visible. Extinction gradual, possibly during a whole second. 1886 Nov. 2, Z. 75°.

B.D. +11°.448, 8·8.—Disappeared at dark limb, which was visible. Hardly instantaneous. 1887 Feb. 1, Z. 43°.

α Tauri, 1·0.—(1) Disappeared at dark limb, which was not visible. Instantaneous; some haze. 1886 Jan. 16, Z. 37°. ☾ centre 12′ S. of star in *latitude*. (2) Disappeared at dark limb, which was visible. Very near N. limb, with which it appeared in contact for a long time without being projected on it more than half of its spurious diameter. Just one second before final extinction it suddenly became much fainter, and did not brighten again. Reappeared at bright limb, instantaneous. Star was occulted for only 13 minutes. 1887 Mar. 2, Z. 74°.

τ Leonis, 5·1.—Reappeared during total eclipse of Moon. Not instantaneous. 2¾-inch refractor: 7 other occultations during the eclipse. 1895 Mar. 10, Z. 65°.

B.D. −2°.3410, 8·0.—Reappeared at dark limb, which was not visible. Not instantaneous. Kellner E.P. 1897 Jan. 22, Z. 69°.

B.D. +6°.2328, 7·7.—Disappeared at dark limb, which was not visible. Instantaneous. The star is double [1″·24, 137°·0, Leip. A. G. C., both 8·0]. It appeared to be single, and although one star must have been occulted some seconds before the other, no degradation of the light was noticed. 1897 April 13, Z. 46°.

B.D. +13°.2100, 8·0.—Disappeared at dark limb, which was faintly visible. Not instantaneous. An 8-mag. star was occulted half an hour before, which appeared to be instantaneous. 1897 May 9, Z. 53°.

B.D. −16°.5833, 7·8.—Reappeared at dark limb, which was visible. The star hung on the limb for a few seconds. 1897 May 21, Z. 78°.

B.D. −13°.3663, 7·5.—Disappeared at dark limb, which was visible. A few seconds before extinction the star suddenly became much fainter and remained so, but seemed to brighten a little just before it disappeared. 1897 July 7, Z. 80°.

Brad. 414, 7·0.—Reappeared at dark limb, which was visible. Not instantaneous. 1897 July 22, Z. 50°.

16 Tauri, 5·7.—Reappeared at dark limb, which was visible. Hardly instantaneous. 1897 July 23, Z. 82°. 18 other occultations observed the same night, but this is the only one that produced this impression.

B.D. −19°.5756, 8·5. Disappeared at dark limb, which was brilliant. Star in contact with the limb for two seconds. 1898 Nov. 18, Z. 73°.

95 Tauri, 6·9. Disappeared at dark limb, which was not visible. Not instantaneous. 1898 Dec. 25, Z. 35°.

27 Arietis, 6·7.—Reappeared at dark limb, one day after Full. Hardly instantaneous. 1899 Oct. 19, Z. 51°.

B.D. −7°.5727, 7·6.—Disappeared at dark limb, which was visible. Hardly instantaneous. 1899 Dec. 8, Z. 60°.

B.D. −10°.3570, 6·0.—Disappeared at dark limb, which was visible. Not instantaneous. Fine definition. Star required about 0·2 second to disappear. 1900 June 7, Z. 69°.

105 Tauri, 5·8.—Reappeared at dark limb, which was visible. The star hung on the limb for several seconds. 1900 Aug. 18, Z. 51°.

Saturn's Ring.—Disappeared at dark limb, which was not visible, except on the planet and ring. Steinheil single achromatic E.P. 155. Some boiling, with steady intervals. The last little patch of light, longer than broad, parallel to the Moon's limb, seemed to linger strangely. The distance of centres was diminishing 0″·28 per second, and the Moon seemed to travel very rapidly across the planet and ring. 1900 Sept. 3, Z. 74°.

π Arietis, 5·6.—Reappeared at dark limb, which was invisible. Not instantaneous. 1900 Sept. 12, Z. 40°.

Harrow, 1901, Nov. 9. G. L. Tupman.

Appulses of Jupiter, Saturn, and Venus.

The late conjunction of Jupiter and Saturn seems to have been one of the most interesting for the last 200 years (see *Observatory*, last April). The two planets are, of course, in conjunction about every twenty years. But it must be rare for Venus to come up to them about the same time, and afford so striking a naked-eye observation as was to be seen on the clear frosty evenings of the 15th and 16th of November. Instances of this might be interesting, if collected. One occurred on February 11, 1524, another twenty years afterwards, on November 11, 1544. In the "Introductory Remarks" in Williams's 'Chinese Observations of Comets,' p. xxv, we have the following:—"In the 17th year, 6th moon, day Ping Seuh, Jupiter, Saturn, and Venus were together in the stellar division Tsau; that is, 1384 July 8. The stellar division Tsau is determined by the bright stars in Orion. The conjunction was most likely in Taurus or Gemini." Our forefathers in England must also have noticed this, for mention is made of it in Stowe's Chronicles, though Venus is not noticed. Stowe remarks:—"The same time the conjunction of two of the greatest planets chaunced, to wit of Jupiter and Saturne, in the moneth of May, after the which a great mutation of kingdomes followed." It will be noticed that the months do not agree in the accounts, but the conjunction referred to must be the same. I have not yet found more recorded instances of the appulse of the three planets ♃, ♄, ♀, though they probably exist. The conjunction of September 1186 does not seem to have been an actual observation, but merely a calculation made by the astrologers.

As to the conjunctions of Jupiter and Saturn, probably one of the closest is mentioned by Simonelli, 'Scientiæ Eclipsium,' vol. iv.:—"An 1563 die 24 Augusti hor. 14. 30' Aurange in gr. 28 Cancri Junctinus vidit Saturnum Australiorem, quasi cooperiri à Jove, ut ipse narrat in Præfatione ad suas Tabulas." This, however, is clearly not a superposition of one planet upon the other.

The Rev. C. Pritchard's reply to Dr. Ideler's conjecture about ☌ of ♃ and ♄ in the year 7 B.C., referred to in Report of Council R. Ast. Soc., February 1857, need not be passed over.

In the volume containing the works of W. Merle, of Merton College, Oxford, whose MS. Journal of the Weather 1337–1344 was published a few years ago, the following treatise is mentioned, 'Pronosticacio coniunccionis magne Saturni et Jovis anno Christi 1365 quam M. W. Red calculauit et J. Asshenden pronosticauit.' Red = Reed, afterwards Bishop of Chichester 1369–1386. The treatise is probably an astrological one.

While on the subject of planetary appulses there is a somewhat wide one, happening under rather peculiar circumstances, which may be here brought forward. A long description of the total solar eclipse of November 20, 1835, in Francis Moore's Almanack for that year, ends as follows:—"Where the Sun is centrally eclipsed on the meridian, the total darkness will continue 4^{m} 33^{s}. It is rather a remarkable coincidence that Saturn will be in conjunction with the Moon on the 18th, Mercury on the 19th, Mars about midnight of the 20th, and Venus on the morning of the 21st. Hence where the eclipse is central these four planets, though so near the Sun, will be, most probably, visible." The central track, according to Moore, was from near Cape Lopez, traversing Loango and Congo, across the Mozambique Channel and the island of Madagascar. It is to be regretted that the age of amateur or of physical observation had not quite arrived then. The only chance that it was witnessed would be if some old Indiaman happened to be passing through the shadow on its outward or homeward voyage. Another noteworthy point is that in the same month there was a transit of Mercury, one of the longest in duration of the last century. But not a single observation of it seems to exist. S. J. JOHNSON.

Melplash Vicarage, Bridport,
1901, Nov. 29.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1902.

GENTLEMEN,—

Two periodical comets are due to return in 1902, both of which are connected with the name of Prof. Swift, though not as the first discoverer.

One of them was discovered by Tempel at Marseilles on the 27th of November, 1869, but its periodicity was not recognized until after its re-discovery by Prof. Swift on the 11th of August, 1880; and, as the period is about 5½ years in length, an unnoticed return must have taken place in the spring of 1875, when its position would not have been favourable for observation. This was also the case in 1886, when the comet was not seen; but it was well observed, though an extremely faint object, in the autumn of 1891, when it was first perceived by Prof. Barnard at the Lick Observatory on the 27th of September, and passed its perihelion on the 9th of November. In 1897 it was again unfavourably placed, but we may hope it will be observed at the next return towards the end of 1902.

A very interesting comet will also be due about the same time, but the hope of its becoming visible is by no means strong. It was discovered by Prof. Swift on the 20th of August, 1895, and calculated to have a period of somewhat more than 7 years. What renders it of especial interest is the probability that it is identical with a comet discovered by Messier at Paris on June 14th, 1770, and calculated by Lexell to be moving in an elliptic orbit, with a period of 5½ years, so that a return was expected in the winter of 1775. This prediction did not come off, and the subject of it was in consequence called Lexell's lost comet. The cause of that and subsequent failures to appear was that the comet made several close approaches to Jupiter, which greatly changed its orbit, particularly in 1779. If, as Dr. Schulhof thought, the comet detected by Prof. Swift in 1895 is really identical with the long-lost body, and its period is now 7 years, a return will be due, under unfavourable circumstances, about the end of 1902, and another, which we hope will be more favourable, early in 1910.

Yours faithfully,
Blackheath, 1901, Dec. 2. W. T. Lynn.

Anomalous Occultations.

Gentlemen,—

I have just read Professor Davis's letter in your November number, with reference to occultations of stars by the Moon.

The question is, what will happen at the time of occultation if a star is really double? If, when the first component is occulted and the second is still say 5″ distant from the limb, very obviously a second occultation will take place about 10 secs. later. But if the distance is only one-tenth of 5″, the two occultations will now be only about 1 second apart, and so on.

This is no theory or hypothesis—it is a *vera causa.* And it serves to explain in a natural manner the anomalous occultations observed by Tebbutt and many others. Many of the stars actually so observed have afterwards been found to be double stars.

The atmosphere has very little effect on occultations as far as the moment of disappearance is concerned. I have observed in all states of definition. Anomalous occultations occur under the best circumstances of definition, and *per contra* instantaneous disappearances under the very worst, as when the Moon is near the horizon and the star-disc swollen to some 8″ or 10″ diameter.

We possess a valuable means of detecting those very close double stars which are in the Moon's path through their occultations. This method is strongly recommended for trial to observers who possess a telescope but no clock. After a little experience of occultations at the dark limb, the observer will find that most disappearances are instantaneous or nearly so, but that some are decidedly not instantaneous; in others, again, the star really glides away, sometimes in two distinct stages. The latter cases should be carefully recorded. The approximate time and the magnitude of the star will generally serve for identification, but in a crowded field the positions of neighbouring stars by alignment should be noted.

Yours &c.,
Cape of Good Hope, R. T. A. Innes.
1901, Nov. 27.

The Equatorial Markings on Jupiter.

Gentlemen,—

In the present year the low position of Jupiter (23° S. Dec.) has naturally acted very prejudicially upon the definition of the markings. During the five months from the end of May to the end of October I obtained observations of the planet on 76 nights, but there were only three or four occasions when the image was really good and sufficiently sharp and steady to enable the more delicate features to be well observed. With a 10-inch reflector, power 312, I took 520 transit-times of various spots, chiefly of the dark and white patches alternating on the northern edge of the great southern equatorial belt. During the three preceding years, 1898, 1899, and 1900, this equatorial current exhibited a mean rate of $9^h\ 50^m\ 24^s$, or 6 seconds less than the period of $9^h\ 50^m\ 30^s$ on which System I. of Mr. Crommelin's ephemerides is based. But the observations of the present year have shown that a considerable retardation has taken place, for the spots have been running nearly level with the ephemeris times. As examples, I give the individual observations of a white spot situated near the zero meridian, and of a dark spot immediately following it :—

	White Spot.			Dark Spot.		
	Transit-time.		Longitude.	Transit-time.		Longitude.
1901.	h	m	°	h	m	°
May 28	14	6	1·2	14	25	12·8
June 27	12	15	354·5	12	50	15·8
July 4	...		...	12	5	14·6
18	10	21	3·3	10	34	11·2
20	...		...	11	49	12·8
Aug. 1	8	58	4·0	9	15	14·4
8	8	5	357·1	8	23	8·1
10	9	24	1·2	9	42	12·2
15	7	38	6·0	7	56	17·0
20	...		...	6	8	20·5
22	6	50	1·8	7	18	18·8
24	8	3	2·0	8	23	14·1
Sept. 9	7	56	2·8	8	17	15·6
14	...		...	6	28	18·0
18	8	37	7·8	...		...
21	5	31	7·6	5	52	20·4
28	...		...	5	7	17·2
Oct. 7	5	18	3·4	5	37	15·0
30	4	30	1·1	4	51	13·9

Thus during the five months there occurred no material changes in the longitude of the pair of markings other than those introduced by errors of observation and by minor irregularities in the motions of the objects. During the period alluded to there was another prominent white spot in long. 27°·4 (14 obs.) and a dark spot in 40°·2 (16 obs.). The whole of the observations include 28 equatorial spots, but the materials are not yet reduced. Some of the objects have exhibited a rotation-period slightly exceeding $9^h\ 50^m\ 30^s$, while that of others has been less.

I had been hoping to continue these observations through November, but telescopic work has rarely been possible, owing either to frequently dense fogs accompanied with severe hoar-frosts, or to gales of wind with rainy, overcast skies. With the naked eye, however, I have had two interesting views, viz., one of Mercury, shining conspicuously in the morning twilight of

November 16, and another of Venus, Jupiter, and Saturn near together in the S.W. sky on the very clear evening of November 17.

Yours faithfully,
W. F. DENNING.

Bishopston, Bristol,
1901, Nov. 20.

The Council of the Royal Astronomical Society.

GENTLEMEN,—

May I suggest that it would materially assist the Fellows of the Royal Astronomical Society in voting for the Officers and Council if a list of attendances of the Candidates were published, inasmuch as they would hence gather who those members are who sufficiently regard their interests to be present at the Council Meetings.

I am, faithfully yours,
WILLIAM NOBLE.

Forest Lodge, Maresfield, Uckfield,
1901, Dec. 26.

Life on Mars.

GENTLEMEN,—

May I reply through your pages to the following criticism of my elementary Text-book of Astronomy contained in your November issue?

Referring to my lines, "it is an abuse of language to say that life in Mars may be utterly unlike life on the Earth; if it is absolutely unlike it is not life, whatever else it may be," it is objected that this is mere play upon words and "simply depends on Mr. Comstock's definition of 'life,' which is by no means a necessary and sufficient one." The point here at issue is one of logic rather than of astronomy; but in the interest of sound thinking it seems desirable to clear the matter up, and I therefore reply that I have given no definition of life, but have contented myself with stating certain limitations within which, it appears to me, any satisfactory definition of that much-debated term must be included. But whether the reader agrees with me in these limitations or dissents from them is a matter of complete indifference for the present purpose, since the proposition above criticised is not in any way dependent upon them, but is based solely upon the logical principles to be observed in connection with classification and definition.

The ancient rule that a definition should be *per genus et differentiam* commands my adherence, and in accordance with it one is compelled to find as a preliminary to any definition of life some quality or qualities shared by all objects included within its connotation. But the statement "life in Mars may be utterly unlike life on the Earth" is in express terms a denial of the necessary existence of such qualities and therefore precludes the formation of any *genus* which shall embrace both the terrestrial and extra-terrestrial objects under consideration. I must therefore maintain that it is an abuse of language to attempt to unite under one term concepts which, by hypothesis, have no element in common. It is to be understood, however, that neither here nor in my text do I express any opinion adverse to the possible existence of life upon Mars.

I embrace the opportunity to acknowledge here, what the plan and scope of my Text-book did not permit, that the argument with regard to extra-terrestrial life there given is not original with myself, as is implied in the review, but is adapted from Scheiner's brilliant essay, *Die Bewohnbarkeit der Welten*, "Himmel und Erde," vol. iii. p. 67 *et seq.*

Yours faithfully,

Madison, 1901, Dec. 3. GEORGE C. COMSTOCK.

The Colour of Sirius in Ancient Times.

GENTLEMEN,—

This note is merely supplementary to what has been written before on the alleged change of colour of Sirius since ancient times, when it appears to have been of a reddish hue. The first person to call attention to this was Thomas Barker, of Lyndon, Rutland, in the *Philosophical Transactions* for 1760, as referred to by me in the *Observatory*, vol. x. p. 194, and afterwards by Prof. See in *Astronomy and Astro-Physics*, vol. xi. p. 269. Barnard, however, admits that Hyginus was an apparent exception to this testimony of the ancients, because he uses the expression of Sirius "propter flammæ candorem," but he gives quotations to show that "candor" may signify simply brightness, and have no reference to colour. Prof. See (*Astronomy and Astro-Physics*, vol. xi. p. 374) takes the same view, but also argues that little weight should be attached to the remarks of Hyginus, as not being the result of observation. With your permission, I should like to call attention to a passage of Plautus, which seems to have been forgotten in this connection. The prologue of the comedy "Rudens" is supposed to be spoken by Arcturus, and the third line runs:—

> Ita sum, ut videtis, splendens stella candida.

No one ever called Arcturus a white star; and "candida" must be intended to signify "bright," which justifies Hyginus in applying the word "candor" to Sirius, although the evidence seems strong that it was of a red colour in his time.

Yours faithfully,

Blackheath, 1901, Nov. 11. W. T. LYNN.

P.S. (Dec. 13).—Since the above was written I have seen Prof. Newcomb's recent work 'The Stars, a Study of the Universe.' It will be noticed that he still thinks (p. 122) it improbable that a change has taken place in the colour of Sirius within historic times, on the ground that the acceptance of such "would require much stronger proofs than any that can be adduced from ancient writers," remembering not only the vagueness of some of their statements, but "the possible mistakes or misinterpretations of the copyists who reproduced the manuscripts." Proof is perhaps not within our reach, but it seems to me that Prof. See's very full discussion of the subject does make it probable that some change has taken place, and the above quotation from Plautus may be considered to have some bearing upon the matter.—W. T. L.

OBSERVATORIES.

Natal.—The Report from this Observatory of the work done during 1900 shows little difference from that of the year preceding. The Junior Assistant has resigned his post, and the vacancy has not been filled, so that the staff consisted of Mr. and Mrs. Nevill, and Mr. Hammond, the Meteorological Assistant. The entire work done appears to be the keeping of meteorological records and the distribution of time-signals. Some of the instruments and apparatus appear to require repair or renewal; in fact the Director states that the equatorial cannot be used until new illuminating-lamps and batteries are supplied.

The following reports of the work done at various Continental Observatories in the year 1900 have been abridged from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' 1901, part 2.

Arcetri. *A. Abetti.*—Comets and small planets. Longitude work.

Bamberg. *Ernst Hartwig.*—The 4-inch heliograph has been repaired. From May to August Dr. Clements observed the Sun on 84 days, on 44 of which it was free from spots, and took 37 photos; but from August to December it was observed on 45 days only, 26 days without spots, 22 photos. Photos of partial eclipse, 1900, May 28, were also taken. Herr Theile used the large refractor for making spectroscopic observations of Nova Persei. Great attention was given to variable stars, 844 comparisons being made on 153 nights. The variation in brightness of Eros was also well watched, as was that of Nova Persei. Meteorology.

Berlin. *W. Foerster.*—With transit-circle, 420 transits and 200 zenith-distances were obtained. Herr Knorre, with the 9-inch, made parallax observations. Time service.

Bonn. *F. Küstner.*—The Director has made some researches and investigations with the new optical and photographic refractor. The optical objective is 14·2 inches, and will easily separate a double star 0″·5 distance, and shows 0″·3 elongated. On one occasion ϕ Ursæ Majoris was separated with power 680 (on this date the Greenwich observers made it 0″·30). The photographic objective of 11·8 inches gives good round images. The usual meridian work. Variable stars.

Breslau. *J. Franz.*—The revision of selenographical coordinates of 150 bright and small lunar craters, by measuring the Lick plates, was carried on in conjunction with Dr. Rechenberg. Cometary observations. Latitude observations by Talcott's method.

Dusseldorf. *Wilhelm Luther.*—This Observatory is almost entirely devoted to observing small planets.

GENEVA. *R. Gautier.*—The observations of comets and small planets have been published in the *Astronomische Nachrichten.* The total eclipse, 1900, May 28, was successfully observed by MM. Pidoux and Schaer, members of the staff, at Ménerville (Algeria). Meteorology; testing watches.

GÖTTINGEN. *Wilhelm Schur.*—Observations with the large heliometer of 70 Ophiuchi, diameter of Sun, parallax of Polaris and 61 Cygni. Variation of brightness of Eros. The Repsold heliometer was employed by Dr. Ambronn in measuring the Sun's diameter, diameter of Mercury and Venus, and some wide Struve doubles.

PUBLICATIONS.

PUBLICATIONS OF THE LICK OBSERVATORY, VOL. V., 1901.—Readers of the *Observatory* have been prepared for the issue of this volume by several references in our Notes and by the short letter of Prof. Hussey in No. 304. The title "Micrometrical Observations of the Double Stars discovered at Poulkowa, together with the Mean Results of the previous Observations of those Stars," points out that Prof. Hussey has herein done for the Otto-Struve stars what Prof. Burnham at Yerkes did recently for the Burnham double stars. That is, all the stars are arranged in order of Right Ascension, with the annual means of previous observers and those of Prof. Hussey himself. The arrangement and general get-up of the work is similar to that of the Lick Volume III. and of the Yerkes Volume I., and is, on the whole, perhaps, the best possible form, allowing as it does for diagrams and copious notes.

We have felt, and all interested in double-star work have felt, that this method of taking each large and valuable catalogue by itself is the only chance of keeping abreast of the large mass of measures and of economizing the labours of the observers. Prof. Hussey goes to the root of the matter, and we cannot do better than quote some of his remarks, in the hope that they will bear fruit:—

> During the past thirty years the field of investigation has been enlarged by the discovery of nearly four thousand double stars. These include many rapid binaries, and on this account they have given the subject a greater interest, so that the number of observers has materially increased, but not to that extent which makes it easy for them to obtain all the measures desirable of pairs now known. The literature also has extensively increased and is scattered throughout various publications. Hence it is difficult to gather whether an individual pair is or is not in need of measurement, and in consequence many redundant measures are made of some pairs and others get quite neglected. In view of these circumstances it is eminently desirable that these scattered measures should be collected, classified, and printed in a form convenient for reference. The present volume is intended as a contribution in this direction.

Prof. Hussey commenced this work in 1898. The Poulkowa Catalogue of double stars was published in 1843, and really consisted of two classes of stars, viz., close pairs and those between 32″ and 2′ of arc, and naturally the present work is devoted exclusively to the close pairs. In the various revised lists which afterwards appeared, some stars in the original catalogues were neglected, either because the distance was too great, or they were identical with the Struve pairs, or were thought to be single. All these neglected pairs have received attention by Prof. Hussey, and the weight of evidence goes to show that 33 pairs are really single, viz.: OΣ 8, 10, 25, 27, 39, 60, 83, 89, 97, 99, 102, 110, 124, 135, 138, 169, 178, 246, 247, 248, 251, 254, 264, 286, 333, 342, 358, 364, 365, 429, 471, 474, and 491; while new companions have been found to OΣ 190, 247, 283, 361, and 476.

There is, in connection with the measures of Otto Struve, a tough problem which Prof. Hussey has had to face. Every double-star observer knows that Otto Struve made elaborate experiments with artificial double stars, from which he found that his measures were subject to large systematic errors or personal equation varying with the position-angle and distance. He also compared his measures of double stars directly with those made by other observers, and found similar results. But all computers who have had to deal with the measures have found that sometimes the corrected and sometimes the uncorrected measures are closer to the mean values, that correcting the measures merely throws them to the same distance on the other side of the curve, and so on. In the end Prof. Hussey has printed both the actual and corrected measures, leaving it to the computer to make his choice, and we cannot see how he could do otherwise. For those who have not looked at these errors we may mention that they sometimes reach 10° and 11°.

The work is wonderfully free from misprints, the few there are being very obvious and will occasion no inconvenience. On page 126 the last measure should be 320°·8, not 302°·8; on page 144 evidently 1899·75 should read 1898·75; on page 127 the figure for OΣ 288 (which, by the way, is undoubtedly binary) is printed wrong way up, but is easily rectified by writing 0° and 180° for 90° and 270° respectively. The mere fact of dragging in these slips to show Prof. Hussey that his work has been carefully read is sufficient testimony to the great care he has bestowed upon it. Indeed a volume of this kind, to be of use, must be undertaken by one whose heart is in his work, who is himself a skilled observer, and has an intuitive perception of what is or is not a reliable observation. By producing a work worthy of the high traditions of the Lick Publications, Prof. Hussey has shown himself possessed of all these qualities, and we heartily congratulate him on his success.

NOTES.

Minor Planet Notes. — An interesting planet, to which a letter has not yet been assigned, was discovered by photography at Arequipa on August 14 by Mr. Stewart. It was then in

S. Dec. 61½°. A series of positions were secured, from which Prof. Newcomb and Miss Winlock have deduced the following elements:—

Perihelion passage ..	1901 Oct. 9.
ω	301° 19′
☊	35 48
i	18 38
ϕ	22 8
μ	860″
Perihelion distance ..	1·603
Aphelion distance ..	3·541
e	0·3768

The interesting feature is the very large eccentricity, the greatest known among the planets; it is quite comparable with that of some of the short-period comets, the angle ϕ in the case of Holmes's Comet being 24° 17′.

Another planet, HL, was discovered by Wolf at Heidelberg on December 16. A. C. D. C.

Some Startling Meteors.—Reports have come to hand of several remarkable objects of this class, seen during December. On Wednesday, the 4th, about twenty minutes to six in the evening, a meteor was seen by observers in different parts of the country—Margate, Salisbury, Portsmouth, Birmingham, and other places—to pass from near Vega to α Cygni and beyond, which was so brilliant as to light up the scene with the *brightness of daylight*, as one observer says. It left a track 45° or 50° long, which endured for several seconds. Also, it was stated in a daily paper of December 10, that in the early hours of the previous morning a brilliant meteor was seen from Dover in the south-west sky, which appeared as a large ball of light with a luminous tail, and shot through space with enormous velocity, falling into the sea with a high splash.

The Orbits of Binary Systems.—The study of close binaries, whether by means of the spectroscope or by photometric methods, is most interesting and pregnant with possibilities. If Professor Campbell's estimate is correct, we are confronted with the fact that at least one star in every five or six is a spectroscopic binary.

Mr. A. W. Roberts, of Lovedale, has not only made extensive photometric observations of variables, but has investigated methods of deducing the orbits and other circumstances from such observations; and consequently he is in a position to form a fair opinion of our present knowledge of the subject. This makes his remarks in *Nature* (Sept. 12) peculiarly interesting. Of the many problems in connection with these circumstances he considers two of especial importance, as bearing directly on their evolution—*i. e.*, the mean density, and the form of the two components. Professor Russell finds, from an investigation of seventeen out of the twenty-two Algol variables, a mean density of 0·19, the density of water being taken as unity. While Mr. Roberts deduces from a discussion of four southern Algol variables a mean density 0·187, and a later discussion of all the eight southern Algol variables the value 0·176. The practical accordance of these results may be fortuitous; but it is more probable they indicate the truth of the conclusion that the average density of close binary stars is one-sixth that of water. Mr. Roberts considers that any investigation of the form of the two components should deal first of all with the five Algol variables, which revolve in contact. These five are:—

Name.	Period.				Max.	Min.
	d	h	m	s	mag.	mag.
V Puppis	1	10	54	27	4·1	4·7–4·9
χ Carinæ	1	1	59	0	7·9	8·6–8·7
RR Centauri	0	14	32	7	7·4	7·8–7·85
β Lyræ	12	21	46	58	3·4	3·9–4·5
U Pegasi	0	8	59	51	9·3	9·8–9·9

The figure of β Lyræ has been considered by Mr. Myers (*Astrophys. Journal*, vol. vii.), where he shows that each star forming the system is not a sphere, but an ellipsoid of revolution. The amount of flattening is found to be 0·2 in both stars. Dealing with U Pegasi in the same manner (vol. viii.), he finds distinct evidence of ellipsoidal figures in both components.

Mr. Roberts himself, in vol. xiii., gives similar results for V Puppis. He now finds that the stars of χ Carinæ have separated, but that there is not unmistakable evidence for ellipsoidal figure. In the case, however, of RR Centauri, a twin system to V Puppis, the curve indicates without doubt a considerable ellipsoidal form of both stars.

Hence he concludes that from V Puppis on the one hand, a dumb-bell system speeding round in 35 hours, to the twin stars of Castor, completing their great round in 1000 years, we have a regular chain of sequences in distance. The links of this chain are made evident by observation as well as by theory. It is not unreasonable, therefore, that the present trend of astrophysical research should be in the direction of discovering more fully and certainly the different stages of evolution and development in the architecture of the heavens.

The Weather in November and December.—Although subject to frequent and sudden alternations of temperature, November was cold upon the whole, the mean temperature for the month being 2° below its average. The highest temperature in the month occurred on the 11th, and was 54°·8, and the lowest, six days later, was 20°·6, a temperature lower than any recorded at the Royal Observatory in November during the past 11 years. The lowest temperature on record for the month, in the 60 years 1841–1900, viz. 18°·3, occurred on November 28, 1890. The month's rainfall was only 0·67 inch, and the sunshine amounted to 46 hours, the sunless days having been 16 in number. In December the weather was generally mild till the 13th, cold till the 27th, and mild again at the end of the month. The highest temperature was 55°·2 on the 7th, the lowest 24°·8 on the 17th, and the mean 39°·0. Sunshine was recorded during 37 hours, being about 15 per cent. of the possible amount, and the rainfall amounted to about 3 inches.

A New Catalogue of Variable Stars.—A Committee of the Astronomische Gesellschaft, consisting of Messrs. Duner, Hartwig, Müller, and Oudemans, have undertaken the preparation of a Catalogue of Variable Stars. They request observers who have a considerable unprinted series of observations, which would be useful in the correction of elements, to publish them at once or to communicate them to Prof. G. Müller, Potsdam Observatory. The Committee will also undertake the definitive notation of newly-discovered variables as soon as their light-fluctuations are certainly ascertained. They will shortly publish a list of variables discovered in recent years which are unnamed.

Bulletin No. 11 of the Lick Observatory contains a useful set of measures of 194 double stars made with the 36-inch and 12-inch refractors by R. G. Aitken in the years 1900 and 1901. Perhaps the most interesting just now are the measures of δ Equulei, and it is satisfactory to find that so far the measures at Lick and Greenwich are in close agreement. There are a few printer's errors easily rectified, *e. g.* τ Geminorum is β 1009, not β 1007, and the three first measures of β 612 are for 1900, but in the case of Σ 2556 the year is uncertain.

The next Meeting of the Royal Astronomical Society will be on Friday, January 10; of the British Astronomical Association on January 29.

From an Oxford Note-Book.

The Royal Society Anniversary Dinner on Nov. 30 was not quite so well attended as usual, but some excellent speeches were made, especially that of the President. I have often commented on the *Times* report of these occasions, that the really important speeches (those of the President and Medallists, for instance) were condensed to a few lines only, while a few commonplaces, uttered by some politician, were given at full length. On the present occasion, however, the President's speech was well reported. The cynical may say that this was merely because there was no Cabinet Minister present to distract the reporter's attention; but we may hope that he was influenced by higher motives, and recognized a really first-rate speech when he heard it. The Bishop of London, however, said things which the *Times* has not put in. There was a beautiful parable about Science being like a Well: I think it was the Well of Bethlehem; and anyway it was the Well David thought of when fighting and very thirsty, and the mere thought of a drink from it refreshed him. And so, the Bishop said, the clergy, fighting the battle of life, thought of a refreshing draught from the Well of Science: they had not often time to do more than *think* of it, he said, "although I will say," he added, "that we poor, hard-working parsons, *we drink when we can!*" This was applauded about as vigorously as anything said at the dinner; and yet it is not in the *Times* report at all: nor is anything said about the Bishop's immediate assertion that he was a teetotaller and a non-smoker, nor of how he ostentatiously drunk the toast he had proposed in coffee. But as the *Times* has reported the President so well, we must, I suppose, forgive these minor omissions.

THE "Sensational Revelations" of the Reflector at the Yerkes Observatory still continue. I don't remember ever seeing anything so strikingly beautiful as the picture of the Andromeda nebula in the last number of the *Astrophysical Journal.* The other pictures are, perhaps, quite as good, but I should pick out this one as really astounding; and I have reason to know that others far better qualified to judge are of much the same opinion. And the motion of the nebula round Nova Persei—so clearly demonstrated! Dr. Max Wolf has added an important contribution, finding that the motion will actually accord with an explosion at Nova Persei on the right date, within a few days natural margin. We can scarcely, as yet, appreciate the full force of these things; we must wait, as before remarked, to get our breath again. Meanwhile it is excellent news of another kind that a bigger reflector is contemplated. The Director of the Yerkes Observatory is modestly enquiring, by circular letter, whether the form suggested is the best, or whether any other form of telescope is more likely to advance the true interests of Astronomy. When such a question is asked in such a way it deserves full consideration before reply; but, speaking for myself, the reply I should have written by return of post (and may very possibly send ultimately for that matter) would have been to the effect that those who are doing such work as that of which we have recently had specimens are better without any advice. The best interests of astronomy are perfectly safe in their hands.

WE take some comfort in the thought that the Reflector, which is now doing such good work in America, is English-born. *En revanche*, it may be remarked that a native American instrument is beginning to do excellent work in England. I have recently had the pleasure of seeing the working of the Durham Almucantar, and came away with feelings of envy and admiration. Prof. Sampson has now got his printing chronograph (also American-born, and due to Prof. Hough of Chicago); and the complete equipment promises to be as near perfection as the heart of an observer could desire. The surprising thing about the

Almucantar is that obvious difficulties yield one after the other in a most pleasing way; for instance, the important difficulty of wind. One might think that a floating instrument would be affected by the wind in spite of all one could do; but Prof. Sampson has built an experimental wind-screen attached to the *trough* in which the instrument floats; and although a gale was blowing when I was there, I found it quite possible to make observations, though they were probably not good. But the wind-screen was only a trial one, and is to be extended in a manner which will probably remove any appreciable effect.

A GOOD suggestion was made the other day, by Mr. C. V. Boys, about the marking of periodicals on the back. When a volume is made up of (say) twelve separate parts, it is a great convenience to have the numbers 1 to 12 on the *backs* of the separate parts, so that the completeness of the volume can be recognized on the shelf, or a particular number selected without taking down the others. But sometimes the separate parts are too thin to admit of lettering on the back. In that case, said Mr. Boys, why not have a diagonal line across the back of the volume? *i.e.*, a set of marks (of any kind so long as they were conspicuous) occupying a progressive series of places on the backs of the parts. I commend the suggestion, in the first place (following the lead of Charity in beginning at home), to the Editors of this Magazine, and to those of the *Monthly Notices.*

SOME answers to examination questions:—

Find the focus of the small telescope. This is done by placing two very minute threads (obtained from spiders reared expressly for this purpose) at right angles to each other across the bottom of the tube. Where the two threads cross is the focus.

(Of a total eclipse.) Birds go to roost and cattle behave as if night was approaching, and you yourself feels it. . . . The corona can be seen, and this is the chief object that astronomers scour the country at times of approaching eclipse to study.

With reference as to how the spectroscope does split up or analyze light, I have not time to explain; and as the question does not ask me to, I take it you do not require it.

In summer the position of the Earth as regards the Sun is such that, although he is 180 million of miles further off than in winter . . .

At the Equator, every time the celestial globe rotates an entirely different set of stars are seen above the plane of the horizon.

The Horizon is the point where the horizontal line cuts the Prime Vertical at right angles.

IN the Reports of the Seismological Committee to the British Association Prof. Milne has drawn attention to the fact that earthquakes are more numerous when the poles of the Earth, in their complex movements which go by the name of "Variation of Latitude," are moving with greater velocity than usual. The evidence which it has been possible to accumulate up to the present is only meagre, and the connection of these phenomena is only, therefore, a suggestion as yet; but it is a most interesting suggestion, and appeals to one's sense of the fitness of things. The "wabbling" or "wobbling" of the pole (I never quite know how to spell that word) must set up strains in the Earth which might result in earthquakes—cracklings of the strained surface: and I suppose the strains would be greatest when the motion is greatest? This possibly wants more careful investigation.

SOME time back, when Mauritius was a centre of attention owing to the approaching eclipse, an odd advertisement from a Mauritius paper was sent me and printed in these Notes. Its appearance seems to have suggested to a friendly correspondent that more of the same kind might be appreciated; and he has sent the following. But the astronomy, which was meagre enough in the other, has entirely evaporated from this, and I print it apologetically. It is the unusual boast in the last sentence which seems to give it a claim to consideration:—

OUZEER KHODABACCUS.

Since March 1885, Mr. Ouzeer Khodabaccus, the well-known Mahomedan priest, has been authorized by Government to attend patients by means of oral and written prayers.

Before obtaining the authorization, he had previously, for a period of about twelve years, waited upon many patients condemned and given over by Medical practitioners, and succeeded in saving their life, only by means of prayers.

The numerous testimonials emanating from those who have recovered as above, or simply witnessed some of those cures appeared in the *Mercantile Record & Commercial Gazette* of the 14th November 1887, and they speak volume Ouzeer Khodabaccus has not thought it advisable to ask for fresh ones from those cured since.

For forty years past, he daily reported himself (and still does so) to Fort William, and there prays to God that he may avert enemies from Her Majesty the Queen's possession and extend them the new glorious conquests.

Since 1847 he is in Mauritius and has never been tried before any Court of Justice.

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AT the last moment I add a note of admiration for Prof. Kapteyn's brilliant suggestion with regard to Nova Persei in *Astr. Nachr.* No. 3756. He points out that the velocity with which the nebula is apparently expanding will be the same as that of light if the parallax be not greater than 0″·01; and he suggests that the expansion is not that of actual matter, but merely that of the boundary which the light has reached in the months subsequent to the flaring up, before being reflected back to us. If an explosion takes place in a hilly country, we hear the echoes from more and more distant hills in the order of distance, and we are now perhaps *seeing* the *light echoes* of the flare up of last February. The hypothesis fits the known facts with a striking completeness so far, and adds another sensation to the many which the star has already afforded.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXV. FEBRUARY, 1902. No. 315.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 January 10.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. 76 presents have been received since the last Meeting. Of these the following call for special notice:—First, a medallion in Wedgwood ware of Sir Wm. Herschel, presented by Sir W. J. Herschel. This is a modern copy taken from the mould which was made by Flaxman during Sir Wm. Herschel's lifetime. The existence of this mould was not known until quite recently, when it was discovered by Sir W. J. Herschel, and this copy was taken from it. Then there is a copy of a portrait of Galileo (from an original at Florence) which has been presented by Sir W. Huggins. We have also received volume viii. of the Italian National Edition of the Works of Galileo, presented by the Italian Government; and also a further batch of enlarged charts of the Astrographic Chart of the sky, from Paris.

The President. I think we ought to pass a special vote of thanks to Sir W. Herschel and Dr. Huggins for their presents.

Mr. Whittaker, reading a paper on "Periodic Orbits," said that the paper was concerned with two theorems relating to the periodic solutions of the differential equations of Dynamics. These solutions were first brought into notice by Dr. G. W. Hill, who, about the year 1879, published the beginning of a new lunar theory, based on a consideration of periodic orbits. The subject had since been developed by M. Poincaré and Prof. G. H. Darwin. In this paper those periodic orbits were considered which could be described by a body moving under the influence of any number of Newtonian centres of force and attracted by them according to the inverse square of the distance. The method adopted was practically that of selecting, from the aggregate of all closed curves, those which were solutions of the differential equations, and so differed from that generally adopted hitherto, which was to select from the aggregate of solutions of the differential equations those which were closed curves. This departure was connected with the plan adopted of defining the orbits by the principle of least action, instead of by the differential equations themselves.

The first theorem was that if in the plane in which the motion took place two closed curves could be found enclosing each other, and such that a certain function of position was negative at all points of the inner curve, and positive at all points of the outer curve, then a periodic orbit existed in the ring-shaped region between the two curves. This result might be regarded as an analogue in the theory of periodic orbits of those theorems in the theory of equations which furnish the position of the roots of the equation by criteria depending on the sign of various expressions. The second theorem was that the integral of a certain function of position, when the integration was extended over the interior of any periodic orbit, was equal to the number of centres of force enclosed by the orbit, diminished by two. This might be regarded as in some ways analogous to the well-known theorem of Cauchy in the Theory of Functions, which furnished the number of zeros of a function in the interior of a contour by means of an integral taken round the contour.

Prof. Turner. Is the quantity, whose sign was discussed in the first theorem, zero along the periodic orbit?

Mr. Whittaker. Yes; in fact, from its vanishing the differential equations of motion of the body can be deduced. But of course it is easier to find whether a given quantity is positive or negative at a given point than to find the exact point at which it vanishes.

Prof. Turner. I feel bound to say that I am not competent to criticize or to offer any comments upon this paper, but it is of a kind of which we have had very few in recent years at this Society. It is a branch of astronomy which has been making rapid strides in the hands of Dr. G. W. Hill and M. Poincaré, and it is a pleasant thing to have these advances explained to us by one of our Secretaries who is himself responsible for some of them.

The President. I was just going to say something like what Prof. Turner has said, namely, that I am glad Mr. Whittaker has brought this before us so that it will appear in our *Monthly Notices* instead of in a purely mathematical periodical. I should like to ask him whether this system is easier than others for arithmetical computation or whether the interest of this paper is purely theoretical.

Mr. Whittaker. The second theorem that I wrote on the board actually does tell us the region in which the periodic orbits exist.

I did not, as a matter of fact, make any numerical calculation. The interest is not so much to calculate the orbits as to know whether there are orbits, that is to say, what is the general nature of the solutions rather than what the solution is.

Prof. Turner then read a paper by *Major S. G. Burrard*, on "The attraction of the Himalaya Mountains upon the Plumb-line in India." Major Burrard, the Superintendent of the Indian Trigonometrical Survey in India, sent this paper a short time ago through me, presumably because I had the pleasure of meeting him on the occasion of the eclipse of 1898. He was the Royal Engineer officer in charge of the camp which the Astronomer Royal and I occupied at Sahdol, and very comfortable Major Burrard made us in every way. The paper begins as follows:—

> In a paper read in 1895 before the Royal Society the late General Walker advocated the employment of "groups" of astronomical stations for the purpose of eliminating the effects of local attraction at a central station. In the same paper he explained the preponderance of northerly deflections that prevails throughout India by the hypothesis that local attraction is producing a southerly deflection at Kalianpur, *the station of reference of the Indian Survey.* In 1871 Archdeacon Pratt had come to the same conclusion; in his "Figure of the Earth" he argues that an excess of density *must* exist *near* Kalianpur, for "the deflection at Kalianpur," he writes, "is considerably south." Shortly after the issue of General Walker's paper in 1896, Sir David Gill's work on "The Geodetic Survey of South Africa" was published, in which the adoption of "groups" of astronomical stations was strongly recommended. In consequence of the opinions expressed by these eminent writers, and in full accord with their views, we decided in 1898 to determine the actual effect of local attraction at Kalianpur by means of a complete "group."
>
> By the spring of 1899 astronomical observations for both latitude and azimuth had been taken at Kalianpur, and at eight adjacent stations, four being situated at an average distance of nine miles and four at an average distance of thirty-five miles from Kalianpur. A discussion of the results of the observations showed that local attraction was causing a *northerly* deflection at Kalianpur. This unexpected issue created a dilemma; either General Walker had been mistaken in advocating "groups," or his assumption of a southerly deflection at Kalianpur had been incorrect.

Then Major Burrard discloses two remarkable facts:

> First, that two-thirds of the southerly deflections observed in India are situated within a narrow zone, running from east to west between the parallels of 24° and 26° approximately. Secondly, that large northerly deflections prevail from east to west between the parallels of 24° and 18° approximately.

Major Burrard has spent a good deal of time in seeing if this can be explained by a deficiency of matter represented by the Indian Ocean; but the attraction of the ocean had been found by *longitude* observations to be practically zero, and Major Burrard remarks that it cannot be assumed that the ocean has an effect in one direction when it is known that it has no effect in another direction. Major Burrard comes to the conclusion that there is in the middle of India a submerged backbone of mountain, which is shown in a diagram accompanying the paper and is now on the screen. He considers that the chain of mountains follows the contour of the Himalayas, striking north in the west of India. It is a very important matter which Major Burrard brings before the Society in this paper. It is a summary of a large number of details which are supplied by the Survey of India Department, but these would not have come to the knowledge of the Society unless Major Burrard had been good enough to send us an account of his work.

Mr. Knobel. Does Major Burrard give any indication as to the geological conditions of the land?

Prof. Turner. No, I do not think he says anything about that.

Rev. E. Ledger. By a "submerged mountain" does he mean a submerged stratum of great density?

Prof. Turner. He says:

> There appears, therefore, to be some hidden cause which is tending to reduce the northerly deflections at northern stations and to increase the northerly deflections at southern stations; and I have been led to infer the existence of an underground chain of excessive density that crosses Central India for 1000 miles, and which underlies Balasore, Mandla, Bhopal, and Jodhpore.

I think that is all he says as regards that. I omitted one point, and that is the position of this underground chain is to be tested by pendulum observations, which is a desirable thing in itself. I do not think anything more about the nature of the chain is said in the paper.

Mr. Dyson showed on the screen two pictures of the nebulosity round Nova Persei taken at the Yerkes Observatory. He remarked that Mr. Ritchey had kindly sent slides of his remarkable photographs, and although Fellows had doubtless seen the reproductions in the *Astrophysical Journal*, so much more was shown on the slides, he was sure they would be glad to see them. The mirror with which they were taken had a focal length of 93 inches, and was used with an aperture of 22 inches. The first photograph was taken on Sept. 20th, and the second on Dec. 11th. The great changes which have taken place in the nebula between these dates is readily seen.

The President. As Miss Clerke is present this evening, I will take the opportunity to ask her if she will make any remarks on the subject of Nova Persei.

Miss Clerke remarked that it had been perhaps too hastily assumed that the nebulosity round Nova Persei waited to become visible until subjected to long photographic exposures. The first adequate experiments showed it; how long it had been there unrevealed we had no means of knowing. Prof. Kapteyn's and Mr. Wilson's theory of its being a pre-existing dark formation momentarily lit up by a light-wave progressing onward from the star at its maximum, was extremely interesting as a speculation; but its felicities of explanation were counterbalanced by staggering objections.

Mr. Newall. It seems to me that the suggestion made by

Prof. Kapteyn is of great interest and ingenuity. It may not be familiar to all the Fellows, and perhaps I may attempt to indicate it. His idea is that the light which emanated from the Nova and reached us in February was also spreading in other directions from the Nova: for instance, it travelled in the direction of this point (screen); part of it was reflected from matter already existing there, and in consequence of its distance from the Nova, such light only reached us some six months later, in August. This may account for the nebulosity so well seen in Mr. Ritchey's photographs. The motion observed to have taken place between August and November is from a certain point here to a certain point there (screen); and taking into account the fact that no remarkable parallax has been discovered for the star by direct observation, it seems that the distance of this nebulosity from us, if it is in any way connected with the Nova, or with the outburst of the Nova, must be also very great, and consequently any change observed in the position of it must involve very rapid movement. Kapteyn finds that the movement actually observed might be that due to the velocity of light if the parallax of the star was only $0''\cdot01$. It is an interesting point to note that he has probably deduced this parallax from the actual observed motion; but if we deduce it from the angular distance of the nebulosity from the Nova itself, it gives us the same result; that is, the present motion of the nebulosity as observed between August and November is such as would lead us to imagine that the light had originated from the Nova at its outburst. The actual motion indicated on the slides is very obvious if you compare the position of the cusp, for instance, with reference to the stars round about it in the two photographs. Kapteyn's suggestion, then, is that a certain wave front emanated from the Nova at the outburst and spread out, followed by light in the succeeding months, whilst the Nova was bright. We may limit our consideration to light that is included between two wave fronts, viz., one which emanated at the outburst, and the other at a later date—let us say a couple of months later. That spreads from the Nova, as a shell containing the energy of light. I take it that Kapteyn's idea presupposes the existence of nebulous matter in that part of space before the outburst of the Nova, and his view is that it became visible in consequence of light reflected from it as the shell passed it in expanding. He puts forth the idea that the light is probably *reflected*, in rather a vague way, and his account would lead us to believe that he is dealing mainly with the idea of a shell passing outwards with the velocity of light rather than with the mode in which the nebulosity becomes visible. Some twenty years ago, Siemens made experiments (in connection with his regenerative theory of the Sun's heat), which showed that the passage of sunlight through rarified gases produced dissociation; and it seems to me quite a possible explanation of the luminosity round the Nova, to imagine that when this shell of light from the Nova passes through space which is already occupied by gases that would produce nebula and would be called nebula if we could see them, then the energy of light produces dissociation and puts the gases into a state which, when the light has passed by, leaves them ready to combine again with the evolution of light. There are a number of experiments showing that phosphorescence is produced by *electrical* energy passing through rarified gases; but in the Nova nebula we have the case of luminosity, which may possibly be explained by assuming that intense starlight in passing through gases in space produces similar dissociation, and on the reversion of the gases to their original state, luminosity may be emitted. The difficulty that immediately occurs to one is that luminosity produced in this way depends upon the absorption of energy from the shell which is expanding outwards, and this means that the energy which is left to go on, as *light*, is diminished very considerably. Miss Clerke has called attention to the idea that the next few months will show us how far this luminosity will continue in anything like the degree of brightness which it now has, or had in August. It seems already to be fainter, as Miss Clerke has said. Supposing that it grows fainter more quickly than one would anticipate in the diminution arising from the simple spreading out of the shell, then this might possibly be taken as an indication that this luminosity is produced more by some secondary action than by direct reflection from nebular matter. The suggestion made by Sir Norman Lockyer in this matter is also interesting, viz., that when we are dealing with collisions of groups of meteorites, we may get, as it were, interference-effects which may give rise to the appearance of very great velocity.

Prof. Turner. I should like to express my interest in Prof. Kapteyn's suggestion. I do not know how far he alone is to be given credit for the suggestion. If we take the suggestion in a general way as mentioned by Mr. Newall, that is to say, that *something* is propagated with the velocity of light in all directions, I think we must remember the claims, for instance, of Prof. Max Wolf, who, a week or so before Prof. Kapteyn's paper appeared, suggested that some electrical disturbance is the probable explanation, and mentioned also the parallax $0''\cdot012$ it would lead to, and also showed, what Mr. Newall has emphasized, that the actual motion of small parts of nebulosity from one photograph to the other agrees with the motion from the nucleus up to where they now are. I have also learned that in this week's *Nature* there is a suggestion by Mr. Wilson of the same kind; but for my own part it seems to me that Prof. Kapteyn's suggestion has the merit of being the first absolutely simple one, the travelling of *illumination* without any hypothesis about electricity or anything else. I quite recognize the difficulties of supposing the illumination can travel so far without dying out altogether, as Miss Clerke says. In a conversation I had with her just before the meeting, I replied to her thus: "Would you not have to suppose that, in any case, Even taking an electrical disturbance travelling out as suggested, is it not equally difficult to understand why *that* should not die out when the law of spread is the same in both cases?" *

Capt. Noble. I should like to ask whether Prof. Turner does not think that if light-expansion took place in a radial direction the external nebula would not be more symmetrical. In the photograph we saw on the screen there was an irregular loop. Why should the light show as one little patch?

The Astronomer Royal. I was going to mention the same difficulty which occurs to me in this explanation, and that is, that we have a complex structure in the nebula which is maintained at the different epochs. I do not think any explanation has been suggested why this particular form should exist. There is one form in particular—a sort of forked tongue to the right of the centre of the picture—that preserves its shape, but is translated amongst the stars. The photographs seem to show that we are not dealing with a uniform shell. Photographs have also been obtained at Greenwich which support this view. Mr. Dyson has taken much pains in trying to get photographs with the reflector, and they show that this form has been spreading over many months, showing these details.

Rev. E. Ledger. If the light is going further and further from the star in a spherical shell of a certain thickness, and there is a wisp of nebulosity, would not the illumination appear to travel along that wisp although the shell was expanding? Just before the meeting I mentioned it to Mr. Maunder, and also that point which the Astronomer Royal has referred to. So far as I remember, Prof. Max Wolf mentioned that he had measured two condensations, one of which was moving at such a rate that it would take it back to the star by last February; and that the other one, which was moving more slowly, would have carried it back by a date in November. I think the star itself would be about nine thousand times the brightness of the Sun.

Miss Clerke. 5600 times brighter than the Sun.

Rev. E. Ledger. With what parallax?

Miss Clerke. A hundredth of a second.

Mr. Crommelin. According to the results which have appeared, there is a transverse motion as well. It looks more like a whirlpool motion. That might be explained by supposing the nebulosity not to lie in one plane, but to have a twisted helical structure, so that portions which seemed to be equally distant from the Nova are not so in space.

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* [After the Meeting I made the following rough calculation, which seems to show that the faintness of the illumination is not prohibitive. Light takes 8 minutes to reach the Moon from the Sun, 8 months to reach the nebula from Nova Persei. Taking the original flare-up to be 5000 times the brightness of our Sun, the illumination of the nebula should be to that of the Moon as 5000 to $(30 \times 24 \times 60)^2$. The Moon could be photographed with Mr. Ritchey's instrument in (say) $0^s{\cdot}003$. Hence the nebula, if of the same albedo as the Moon, in $0^s{\cdot}003 \times (30 \times 24 \times 60)^2 \div 5000 = 20$ minutes say. The actual exposures required thus seem reasonable on Kapteyn's hypothesis.—H. H. T.]

Rev. E. Ledger. I think Prof. Seeliger has said, in the last number of the *Ast. Nach.*, that he made some calculations as to the illumination of nebulosity by starlight. I believe he came to the conclusion that at the distance at which the outer part of the nebulosity would be there would be sufficient light to produce an impression on the photographic plate.

The President. We can certainly thank Mr. Ritchey for these photographs, which are not only interesting in themselves but have given rise to a most interesting discussion.

Mr. Lewis then read a paper on the binary star Σ 1639, 68 Comæ Berenicis. He explained that, although some double stars had been observed for a period of 70 years and had during that period passed over a comparatively large distance, it was difficult to tell whether the motion was due to proper motion or to orbital motion. In the present instance a straight line represented the observations very well from 1830 to 1888 if direction only was considered; but when the motion along this line was analyzed it proved to be quicker and quicker as the stars approached each other, thus indicating orbital motion. He then showed that recent observations had been properly located in the first quadrant. The period was about 180 years.

Mr. Lewis then explained on the blackboard how, in such cases as δ Equulei, difficulty was experienced in determining the period, although the stars had been under observation intermittently during several revolutions. We wanted in the present instance continuous measures extending over a period of five years.

Prof. Turner. May I ask Mr. Newall if he thinks spectroscopic observations in the line of sight would contribute to the elucidation of this matter?

Mr. Newall said that Prof. Campbell was engaged in making such observations.

The President. The Society will thank Mr. Lewis not only for the actual paper he has given us, but for the interesting account he has given of the difficulties inherent in certain classes of binary stars.

Mr. Dyson then read a paper by *Mr. Robinson*, "On the Comparison of Photographic and Visual Magnitudes of Nova Persei."

A paper by *Mr. Stanley Williams*, on the period and light-curve of the Variable Star Y Lyræ, was then read. The author stated that this star was supposed to be variable with a period of 42 days, or of half that period, or it had been thought that the period might be about twice that, namely 86 days. Mr. Williams had thought it was 42 days, and he made observations in the year 1891 for the purpose of settling this question, and from these he found a period altogether different; that is, the star is quite a short-period variable. Dr. Hartwig, of Potsdam, about the same time was making observations of the same star, and obtained the same result.

Prof. Turner. It may not be out of place, although not directly connected with this paper, to remind observers of variable stars that the present is especially an important epoch for publishing observations which have not yet been published, because a catalogue of new variable stars is in preparation by a committee of the Astronomische Gesellschaft, and it is important that all materials should be used in preparing that catalogue. In the last number of the *Astronomical Journal* there is a paragraph asking observers to communicate recent observations to Dr. Müller, of Potsdam.

The following papers were announced and partly read:—

Major S. G. Burrard. "The Attraction of the Himalaya Mountains upon the Plumb-Line in India."

A. Stanley Williams. "The Period and Light-Curve of the Variable Star 6685 Y Lyræ."

Thomas Lewis. "Σ 1639 Comæ Berenicis."

W. H. Robinson. "Comparison of the Photographic and Visual Magnitude of the New Star in Perseus."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon and of Phenomena of Jupiter's Satellites made in the Year 1901."

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken in 1899–1900."

E. T. Whittaker. "On Periodic Orbits."

The following gentlemen were elected Fellows of the Society:—

Ashton Charles Allen, 34 Kensington Palace Mansions, De Vere Gardens, W.

John Hind Bell, Nautical Almanac Office, 3 Verulam Buildings, Gray's Inn, W.C.

Walter Ernest Besley (H.M. Civil Service), 75 The Chase, Clapham Common, S.W.

James Henry Booth, F.R.G.S., 32 Sunny Road, Southport.

Arthur Thomas Cooper, A.M.Inst.E.E., Rivernook, De Montford Island, Reading.

Joseph Alfred Hardcastle, 38 Eaton Square, S.W.

Charles Sumner Howe, Ph.D., Professor of Astronomy, Case Observatory, Cleveland, Ohio, U.S.A.

William Gylby Hutchinson, Rock Lodge, Roker, Sunderland.

G. F. Johns, Perth Observatory, Western Australia.

James William Lowber, M.A., Sc.D., Ph.D., LL.D., F.R.G.S., F.R.Met.Soc., 113 East 18th Street, Austin, Texas, U.S.A.

H. Krauss Nield, Thorold Grove, Sale, Cheshire.

Capt. Michael Joseph O'Sullivan, 66 Fern Grove, Hartington Road, Sefton Park, Liverpool.

Charles Tallent Spencer, A.M.Inst.C.E., La Quinta, Lyon Road, Harrow.

Rev. Charles Lakeman Tweedale, M.A., The Vicarage, Weston, near Otley.

The following Candidates were proposed for election as Fellows of the Society:—

J. W. L. Child, Vernham, Merton Road, Wimbledon (proposed by Col. Markwick).

J. Comas Sola S. Felipe, 29 S. Gervasio, Barcelona, Spain (proposed by A. Stanley Williams).

Rev. J. H. Haslam, M.A., Vicar of St. Saviour's, Denmark Park, S.E. (proposed by D. G. Simpson).

Joseph Lunt, B.Sc., F.I.C., Royal Observatory, Cape of Good Hope (proposed by Sir D. Gill).

Joges Chondra Ronj, M.A., Professor of Physical Science, Cuttack, India (proposed by M. Chandra Ghosh).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the twelfth Session of the Association was held at Sion College, on Wednesday, the 18th December, 1901, *Mr. G. M. Seabroke*, President, in the Chair.

Mr. Alexander Smith contributed a supplementary paper on "Nova Persei," giving the result of 50 exposures which we had made on the star, ranging from $2^h\ 10^m$ down to 5^m, and extending over the period from March 20 to November 14. In the March photograph the aureola was absent, though there were faint indications of a darkening towards the centre. By August 16 the aureola had become conspicuous, and it had remained so ever since. There had been a gradual increase in the breadth of the aureola in its relation to the diameter of the central image of the star. In other words, as the central image had decreased in size the aureola had not decreased correspondingly. This led him to the theory that, if the central star disappeared, the rays which now produced the aureola would give rise to an image having the appearance of a planetary nebula. This theory appeared to receive some support from the visual observations of Prof. Barnard, who found, when using the Lick refractor, that the focus of planetary nebulæ lay beyond that of ordinary stars.

M. Antoniadi described eight of his drawings giving the recent appearances of the planets Venus, Mars, Jupiter, and Saturn in 1901. The most interesting facts in connection with Mars during the last apparition of the planet were the faint half-tones of the Northern Hemisphere, and the remarkable doubling of the Cerebus and Styx. The Red Spot of Jupiter, whose drift in longitude was so striking during these last years, has remained almost stationary since October 1900, when its longitude was 45°. A most abnormal swelling of the South Tropical Belt was seen invading the south temperate zone last summer; it began to be visible very early in the apparition, but was especially conspicuous in September, when its intensity almost equalled that of the Great Belt. Saturn did not offer any striking peculiarities this year. The drawing of Venus was taken with the 9¾-inch Cooke English equatorial of Mr. Chatwood's observatory, at Worsley, Manchester.

The President said it had not been possible to have M. Antoniadi's diagrams photographed in time, so that lantern-slides might be prepared for the Meeting; but he hoped that at a later date they might be thrown upon the screen. He asked if M. Antoniadi had anything to say on the "aureola" round the Nova.

M. Antoniadi said that the nature of the "aureola" had been so very thoroughly investigated by Prof. Max Wolf, of Heidelberg, that he had nothing to add to what was already known on the subject. The valuable experiments of Mr. Alexander Smith confirmed, he thought, very conclusively, Prof. Wolf's statements.

Some discussion ensued concerning the aureola, and particularly as to the verification of the light from the two aureolas of the Nova, producing respectively the bands in the spectrum-photographs.

Mr. C. Thwaites asked what was the cause of the curious cutting in half of the aureola. An ordinary object-glass, if cut in half, gave a circular image. Why should the cone be cut in half?

The President said if they took a circular object-glass, bringing the rays of light from a star to a point, they had a cone of rays coming practically to a point; but if they took half that object-glass, they would have only half a cone of rays, and any section of that half cone was a semicircle.

M. Antoniadi explained, by a diagram on the blackboard, how, through the action of the flint, the rays of high refrangibility giving rise to the "aureola" have their focus farther from the object-glass than the plate, and how, by stopping one-half the aperture, a semicircular "aureola" round a circular-star image is obtained. In that case the semicircular "aureola" is the section by the plate of a half-cone at right-angles to the optical axis, while the half-cone of ordinary rays, having its apex on the plate, gives rise to a small circular dot. The discussion was continued by the President, and Messrs. Holmes, Ellis, and Maw.

In a paper on "A Dark Spot in Jupiter's South Temperate Zone, 1901," *Mr. W. F. Denning* said that among the more prominent features exhibited by Jupiter during the opposition of 1901, one of the most interesting was a dark irregular spot situated in the south temperate zone, and in approximately the same latitude as the Great Red Spot. It seemed to have been first observed by Mr. Scriven Bolton in the middle of May. The apparition was significant in connection with the theory of recurrent disturbances on Jupiter.

The Rev. T. E. R. Phillips inquired whether this spot was not the same as the one shown on M. Antoniadi's drawings just exhibited.

A paper on "Photography with a Small Telescope" was contributed by *Mr. John Grigg*, of Thames, New Zealand, and two photographs of the Great Comet, by Mr. Grigg, were thrown upon the screen. The comet was in Orion, near the belt. The photographs showed the dark rift in the principal tail, also the remarkable long, faint, lateral tail. [*Mr. Geo. Bruford*, who had just returned from Australia, described the appearance of the comet, and referred to the gradual closing up of the tails, which had at first been wide apart.]

Notes on a brilliant fireball of December 4 were presented by *Mr. Walter E. Besley* and *Mr. W. C. Tetley*, after which a paper on the "Apparent Thinning-out of the Stars," contributed by *Mr. J. E. Gore*, was read. The writer referred to the table given by Mr. G. J. Burns in the July number of the *Journal*, showing all stars brighter than the sixth photometric magnitude, with the "indicated density" for each magnitude, and said that to extend this to the seventh photometric magnitude, he (Mr. Gore) had made a careful enumeration of all stars from 6·0 to 6·99 in the Harvard *Photometric Durchmusterung*, recently published. As Mr. Burns said, "the fact that the indicated density is always less than unity can hardly be due to any other cause than actual thinning-out of the stars as the distance from us increases," and tended to show, as he (Mr. Gore) had always contended, that our visible universe is finite in extent. His views on the subject were strongly supported by Dr. Roberts's photographs.

In a further paper on the same subject, *Mr. G. J. Burns* tabulated the values of the thinning-out index for magnitudes 2 to 15, distinguishing those obtained by photographic methods, and said that a glance at the table showed how steadily the thinning-out index diminished as we went down the table. If the numbers were plotted on squared paper, it would be seen that the indices were grouped round a line which would, if produced, cut the axis of magnitude at about 20. This signified that if the stars below the 15th magnitude thin out at the same increasing rate, they ceased altogether at the 20th magnitude. Dr. Roberts's photographs seemed to indicate that stars ceased altogether at about the 18th magnitude. The total so obtained was about a hundred millions.

Mr. Crommelin said it was a little difficult to see how there could be a definite limit to the magnitude of the stars in our universe. Even if we supposed that the Universe had a definite limit in space, it would seem that, if we had stars in it of all possible ages, there must be some stars that were once brighter but had now reached such a faint condition that, although comparatively near us, they were of a very low order of magnitude. So long as they were giving out any light at all, they would belong to some definite stellar magnitude, and he did not see how they could draw a definite line, and say there would be no objects below a definite limit of magnitude.

Mr. Holmes said that, supposing we had stars extending unlimitedly in every direction, was not the result found just what we had reason to expect from the certain knowledge that space was also filled with as infinite a number of minute dark particles, which must stop light and eventually act as a fog, cutting off everything beyond a limit that no instrument could be made to pass?

Mr. Wesley thought there must always be a limit to length of exposure in photographing the stars, because there was a certain amount of light in the sky, and when that began to take effect it was necessary to stop the exposure, or the plates would be fogged. This might prevent the faintest stars being photographed; but it did not follow that there were no more stars in existence.

Dr. Smart submitted an approximate ephemeris for Halley's Comet, which was expected to return to perihelion about May 23, 1910.

The Meeting concluded with the reading of a paper by *Mr. W. Alfred Parr*, on the annular eclipse of the Sun, Nov. 11, 1901, observed in Florence as a partial eclipse under exceptionally favourable weather-conditions.

THE ROYAL METEOROLOGICAL SOCIETY.

The Annual General Meeting of this Society was held on Wednesday evening, the 15th of January, at the Institution of Civil Engineers, Westminster, *Mr. W. H. Dines*, President, in the Chair.

The Secretary read the Report of the Council for the past year, which showed the Society to be in a satisfactory condition, there being an increase of twenty-eight in the number of Fellows.

The Symons Gold Medal, which has recently been founded as a Memorial to the late Mr. G. J. Symons, F.R.S., the distinguished Meteorologist, was presented to Dr. Alexander Buchan, F.R.S., for his work in connection with Meteorological Science.

The President, in his Address, dealt with the "Theory of Probability applied to various Meteorological Problems." He considered that for all practical purposes weather-conditions may be looked upon as purely accidental, and that we may apply to them the laws of chance. They are not by any means in reality a matter of chance, for, although we cannot discover it, there is doubtless a cause for each kind of weather, normal or abnormal. After speaking upon the subject of weather-forecasting he dealt with the question, How long is required to obtain a true average? He has come to the conclusion that 10 years' temperature-observations give a mean of which the probable error is a little under one degree; 30 years reduce this to half a degree; 50 years to one-third of a degree; and 100 years to one-quarter of a degree. After dealing with barometer-observations and rainfall he proceeded to speak of weather-almanacs, cycles, &c. In conclusion he said:—"Meteorology is far more than a statistical science, and is very closely dependent upon theoretical mechanics and thermo-dynamics, and in the application of these subjects to meteorology lies the best hope of success."

The Council for the ensuing year were then elected, *Mr. W. H. Dines* being the President, *Mr. F. C. Bayard* and *Dr. H. R. Mill*, Secretaries.

Note on Mr. de Sitter's recent Paper *.

It is far from my wish to extend this discussion beyond the limits of its interest and use; at first I hoped it might cover several of the points of interest connected with the determination of the constants of Jupiter's system; but it has turned upon two only, and it does not now seem expedient to introduce fresh ones. I shall merely add a few words on the matters before us.

Mr. de Sitter enquires how I derive a value 1/13·492 for Jupiter's ellipticity from the quantity which Souillart quotes from Damoiseau's *Mémoire Manuscrit*, $J = \cdot 0247187$. The answer is simple: my authority for this number is Damoiseau himself, who states it on page ii of the Introduction to his *Tables Écliptiques*. I treat it as a number familiar to any student of the subject, rather than calculate the ellipticity afresh with debateable data; but if its use in my argument appears to Mr. de Sitter unfair to his position, I regret it, for the alternative values which he accepts would have served my purpose, since they also are in marked excess above the most reputable direct measures.

For the only point is whether Jb^2 appears to be so well determined that we may properly disregard a correction to it. To justify such a course Mr. de Sitter collects the different determinations and exhibits their agreement with one another; and if we look at the numbers which he brings together I think that all who know the subject will admit that when they do agree their unanimity is wonderful. Unfortunately it is also fallacious. It is true Laplace's value, 0·02190, and that derived by Dr. Cohn, 0·02095, confirm one another well enough; but in Damoiseau's Introduction there are materials for inferring two other values for Jb^2, which differ materially from these and still more from one another: the first of these was that used provisionally by Damoiseau—one of the data "qui supposent les constantes de Delambre"; it is to be inferred from the stated ellipticity, 1/13·492, and, as Bessel showed*, this number, in conjunction with the constants of Delambre—that is to say, of the *Mécanique Céleste*—leads to the value 0·02472. This is the number which Souillart afterwards found in Damoiseau's *Mémoire Manuscrit*, and it must be compared with Laplace's value as it stands, for all depend on the same constants. Damoiseau afterwards adopted constants of his own, and left materials on pp. i & ii of his Introduction from which the corresponding value of Jb^2 and other quantities may be inferred; from Souillart we learn that he changed Jupiter's diameter from Laplace's value 39″·00 to 36″·74, and the value of Jb^2 was worked out by Adams† and proves to be 0·02166. Now Mr. de Sitter supposes $Jb^2=0\cdot2472$ to have been associated with diameter 36″·74, and hence multiplies Laplace's and Cohn's values by $(39\cdot00/36\cdot74)^2$, before making a comparison. In place of doing this they should have been taken as they stand, and Damoiseau's other value should have been included after reducing it by the factor $(36\cdot74/39\cdot00)^2$. We then get four comparable quantities, 0·02190, 0·02095, 0·02472, 0·01923. The third and fourth are two reductions of the same observations by the same calculator, and they differ from one another by one-fourth part of their mean. Mr. de Sitter has stated that Jb^2 is one of the few constants of which more than one reliable determination exists. I think it is pretty clear from the above that they are as much at sixes and sevens as any other element.

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* See *Observatory*, December 1901, January 1902.

We must now return for a moment to the effect of a correction to Jb^2 upon the mass. I do not propose to follow Mr. de Sitter into the shadowy region of forecasts of probable errors. Recalling the equation

$$3\,\frac{da_i}{a_i}=-\frac{dM}{M}+\frac{d(Jb^2/a_i^2)}{1+Jb^2/a_i^2},$$

and taking

$$M=1048,\quad Jb^2=\cdot0247,\quad a_1^2=32\cdot4,$$
$$a_2^2=82\cdot3,\quad a_3^2=209\cdot1,\quad a_4^2=647\cdot2,$$

it is clear that to neglect a difference equal to that actually found between the two values implied in Damoiseau's Introduction is equivalent to neglecting 0·20 in M whenever a_1 occurs, 0·08 for a_2, 0·03 for a_3, 0·01 for a_4. To forecast the effect of so complicated a neglect upon the resulting value of M appears to me very doubtful; Mr. de Sitter's argument makes it quite insensible. An argument is for those whom it convinces. If it were my own case I should not feel altogether at ease in simply arguing away quantities so involved and in some respects so considerable, which might, moreover, without any difficulty, have been included in the solution.

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* *Astronom. Untersuchungen*, ix. vol. ii. p. 63.

† 'Collected Papers,' ii. p. 191.

But Mr. de Sitter urges that no reliable value of Jb^2 could have emerged from his observations. For curiosity I have solved Bessel's four determinations of the mean distances for the value of Jb^2 as well as that of M, and they give $Jb^2=0{\cdot}0107$ to $Jb^2=0{\cdot}0131$, according to the way they are weighted; it seems quite possible that the admittedly finer Cape observations would have given a value that was not altogether contemptible, had Mr. de Sitter given them the chance. But the goodness or badness of the value of Jb^2 is not altogether to the point; the point is that unless the work is so arranged as to permit the solution to separate the correction of M from that of Jb^2, the latter simply remains behind to vitiate the former.

With the rest of Mr. de Sitter's paper I have little to do; to those who have the curiosity I must leave to discover what ground there may be for a flat contradiction of my statement that Bessel's 'Bestimmung der Masse des Jupiter' determines the four mean distances independently; for I can find none myself. Whatever may be their value, they are there, and can be used, as I have used them above, for finding Jb^2, or for any other purpose, besides the determination of M.

As to my other point, that Marth used a wrong formula, and ignored sensible perturbations, in calculating the mean distances, there seems nothing now to add; for it is admitted in full, and all Mr. de Sitter's definitive results have been recalculated in consequence.

Durham, 1902, Jan. 13. R. A. Sampson.

[We are obliged to Prof. Sampson and Mr. de Sitter for this interesting discussion, which has, no doubt, proved instructive to our readers; but we hope that Mr. de Sitter will not think it necessary to add anything further.—Eds.]

The Scientific Life[*].

Our first anniversary in a new century brings us to a parting of the ways. Behind us lies, in the clear perspective of recorded events, the progress and work of the Royal Society from its foundation two and a half centuries ago; before us, even the immediate future can be but faintly outlined through the morning mist of a new era. Surely no one of the founders of the Society would have been bold enough to forecast the marvellous "improvement of natural knowledge" which, under the influence of the Society, has been won by methods of experiment and induction.

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[*] An extract from the *Times* report of Sir William Huggins's speech at the Anniversary Dinner of the Royal Society on Nov. 30 last.

To some of the founders would have been not less surprising the conditions under which this great work has been accomplished. The master minds of that age were more or less under the influence of the ideas represented by the monastery and the cell—that is to say, an academy in which the Fellows live apart from common life, and are secluded from its cares and interests. We find these ideas in Bacon's "Salomon's House," in his classical fable of the new Atlantis; even more strongly in the generous plan for a scientific college submitted to Boyle by the noble-hearted Evelyn; and in Cowley's proposition for a college of experimental philosophy. Now the great work of the society has been done not in the seclusion of an academy, but, so to speak, in the world. The Fellows have not been supported in a learned leisure by the Society, but, taking their full part in the work of the world, of their own substance maintained the Society. Under these circumstances there was no need to limit the number of Fellows to the 27 fathers of Salomon's House, or to Cowley's 20 philosophers. The Society's 450 Fellows, all taking their part in the common life of the nation, are a great power, each Fellow acting upon the men around him, and so the Society, like a leaven, leavening the mind of the people with the vivifying ferment of natural knowledge. And I think upon the Fellows themselves the living in constant touch with the needs and activities of common life may well act as a stimulus to that alertness of mind which is most favourable to scientific progress and discovery. Still, after all, in principle the older men were in the right, for the idea underlying the academic grove and the cloister, and for which these external conditions of life were then considered necessary—namely, simplicity of living and absolute devotion to the pure quest of truth, unswayed by the glittering tinsel of social distinction and success, are precisely those conditions of being which find access to nature's most secret places. A distinguished Fellow of this society, himself an illustration of this spirit, who by his researches has illumined and advanced almost every branch of physical science, told us at our anniversary two years ago, "that a man who is able to devote himself to the study of nature has as good a position as the world is able to confer." Lord Rayleigh is surely right—there is no higher position to which he can aspire. It is not needful in such an assembly as this to dwell upon the more outward and visible signs of the great advance of science, in which the Royal Society has taken a leading part, such as the railroads, which have brought us from our homes, or this light, which makes the night almost as cheerful as the day. I wish rather to ask you to consider, what is not less real, though it may be less in evidence—the inward and intellectual change in the way of looking at things in all departments of thought, which has been brought about by experimental science, to an extent before unknown, each man thinking for himself; no longer sluggishly accepting the dogmas of the past, but, as far as may be possible, proving all things by the Lydian mode of experience and observation. Surely this indirect outcome of the Royal Society's work upon the mind of the people is a contribution of not less price than the direct "improvement of natural knowledge," marvellous as it is, which the Society has accomplished. The extent to which this leaven of the scientific spirit has spread among the people is shown by the number of daughter societies, each devoted to a single branch of science, which have risen up about the Royal Society. It is also shown by the wonderful activity of the scientific press. There is scarcely a department of science, however small, which has not a serial to record its progress, and which does not require a separate library to hold the books written about it. Surely, as Fellows of this Society, at this parting of the ways, at the opening of a new century, we may turn from the past with no little pride. The Society has never before stood so high as at the present time with regard to its scientific activity, and to the number and quality of the papers published in its 'Proceedings' and 'Transactions.' As the day of the new century breaks, natural knowledge will increase, and with the growth of knowledge power over nature. Many diseases will be arrested, and man's needs more directly met out of the bountiful stores of nature. But more precious still to us should be the higher intellectual joys and the more profound mental conceptions which must accompany a fuller revelation of the divine thought in nature.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Reduction of Hornsby's Observations.

GENTLEMEN,—

In the December number of this magazine, Dr. A. A. Rambaut, Radcliffe Observer, in an appreciative review of the work done by Dr. Downing in re-reducing Taylor's Madras Catalogue, again calls attention to the desirability of reducing and publishing the observations made by Drs. Hornsby and Robertson, Savilian Professors of Astronomy at Oxford and also Radcliffe Observers (at that time the positions were held together) from 1774 onwards. Dr. Rambaut and I are both successors to these able men in different ways (the office of Radcliffe Observer having been separated from the Professorship of Astronomy in 1839 by the action of the Radcliffe Trustees, who have complete control of the Radcliffe Observatory independently of the University), and it will therefore be readily conceived that I have no lack of sympathy, in the abstract, with the reduction and publication of observations in which I have an equal sentimental interest with Dr. Rambaut. I must confess that I do not regard the question of the value of old observations as altogether clear, but I do not wish to raise it now. For the present purpose I wish to recognize that Dr. Rambaut has earned our gratitude by demonstrating the value of these observations, and to range myself alongside him in the hope that they may be published.

I have now to add that it has been my ungracious lot to oppose the particular scheme suggested by Dr. Rambaut for publishing them, and I think the time has come for explaining my reasons publicly. (They have been fully explained to Dr. Rambaut himself, without eliciting any reply from him.) In his article in the December number of this magazine he rather gives the impression that the difficulties in promoting this publication have been of a quite general character: "Are astronomers content," he writes, "to leave all this material unused, to moulder in oblivion, while more recent and possibly less accurate observations are submitted to a costly and laborious re-reduction?" This should be the language of a man who has tried various methods and made several applications without success; but, so far as I know, Dr. Rambaut has hitherto made only one specific application, to which there are, at least, two grave objections; these have been pointed out to him and he has offered no reply, but merely repeated his application in the same terms.

The application is referred to by him in the last Report of the Radcliffe Observatory (see *Mon. Not. R. A. S.* lxi. p. 227). It is there stated that an applicatien to the Government Grant Committee of the Royal Society had not succeeded owing to the magnitude of the sum asked for (£2000). The magnitude of the sum is certainly a difficulty: it is half the total annual grant for all the sciences put together. But this is by no means the only difficulty: there are others connected with the manner in which the sum was to be spent. It was to pay the salaries of two computers for ten years, at £100 a year each. Now this is quite inconsistent with fair market-prices in England, and especially in Oxford. The Greenwich system of taking bright boys, of about 14 or 15, for a few years to do computing is pretty well known. Their salaries are £36 a year at first, and are gradually raised in recognition of merit and experience: but they never come near £100 a year for mere computing, though a boy who learns to observe with various instruments and does a good deal of extra work with them may, perhaps, get nearly this amount towards the end of his stay. The Greenwich price of good, accurate, straightforward computing might be put at an average of £50 a year. In Oxford the market-price is even less. When I came to Oxford some years ago and adopted the Greenwich plan of getting boys from school (one of the Choir Schools actually) to do the work of measurement and reduction of the Astrographic Chart plates, I started with the Greenwich figures—£3 a month. But it was pointed out by my Board of Visitors that boys of the same class, employed at the Bodleian Library on work of a comparable kind, only got £2 a month; and that I must not spoil the market-prices for others.

Matters were arranged accordingly: commencing salary £24, gradually raised; and I do not think it is unfair to put the average price of *good* computing in Oxford at £36 a year, or even less. There is, according to my experience, excellent material constantly to be had at this price. Those who come spend a few years between school and some other employment in doing this work, which they do well; and though it is not always directly useful to them afterwards, they learn accuracy and regular habits of work.

Now I have no hesitation in saying that the reduction of Hornsby's observations—the mechanical part of it, of course—could be done thoroughly well in this way. If Dr. Rambaut wants two computers for ten years, the proper sum to apply for is £720, not £2000. How am I to support an application by him for £200 a year to pay two computers, when, for the last six years, I have been receiving from the very same Board a grant of only £150 to pay an average of five computers in the very same city. How are others to get work done in the same city if Dr. Rambaut spoils the market altogether in this way? These questions have been put clearly before him, and I understood at one time that he recognized their importance, and would endeavour to modify his application accordingly: later he briefly informed me that he had altered his mind, but I have not had any indication as to his reasons for so doing.

There is another question concerning this work which has not yet been satisfactorily answered to my knowledge. Is it impossible to do these reductions, or at any rate a considerable part of them, as the ordinary work of the Radcliffe Observatory? Sir David Gill has set an excellent example, at the Cape Observatory, in stopping current observations in order to bring up arrears. Could not the current transit-circle observations at the Radcliffe Observatory be stopped for a few years in order to bring up these important old arrears? I might put the question to Dr. Rambaut in his own words very slightly modified: "Is he content to leave all this material unused, to moulder in oblivion, while recent and possibly less valuable observations are submitted to a costly and laborious reduction?"

These are the chief reasons which were given by me, as a member of the Government Grant Committee, for not supporting Dr. Rambaut's application: that it was on an extravagant scale, and that no good reason had been given why, at any rate, a *part* of the work should not be done in the ordinary course at the Radcliffe Observatory. I am sorry to have to discuss such a matter in public; but private discussion has failed; and I prefer to state the matter thus fully rather than risk a possible misapprehension, on the part of foreign astronomers, as to our general attitude towards these observations.

University Observatory, Oxford,
1901, December.

Yours faithfully,
H. H. TURNER.

Joseph Fraunhofer.

GENTLEMEN,—

Prof. Turner, in his 'Modern Astronomy,' dwells upon the revolution (and, indeed, it is nothing less) which astronomical science has undergone during the last thirty (may we not rather now say forty?) years. The greatest factor in this marvellous extension of the boundaries of our science, which, formerly chiefly mechanical, now trenches upon the domains of chemistry, and renders the expression "physical" no longer appropriate as applied to gravitational astronomy, has been, of course, spectrum analysis, which began by the investigations of Kirchhoff and Bunsen in 1860. But, of course, these were founded upon the knowledge of the fixed lines in the solar spectrum. Newton, when he first

"untwisted all the shining robe of day,"

does not appear to have seen any of these, perhaps because his spectrum was not sufficiently "pure," or perhaps from not (so far as we know) looking at it directly [*]. It was left, then, for W. H. Wollaston, exactly one hundred years ago, to call attention to four of these lines, to which, however, he attached no particular significance, beyond the fact that they marked off definite divisions in the spectrum. Twelve years later Fraunhofer perceived how important their observation might become; in 1814 he observed and measured the positions of a large number, naming them by the letters of the alphabet, the principal ones bearing the capital letters A to H. Since then it has been customary to call them the Fraunhofer lines; and perhaps a few words respecting the personal career of the discoverer and his early difficulties may, at this time, be interesting to some of your readers. They are derived from Utzschneider's 'Kurzer Umriss' [†], an abstract of which is given by Schumacher in vol. v. of the *Astronomische Nachrichten.*

Joseph Fraunhofer was born at Straubing, in Bavaria, on the 6th of March, 1787, the year after Sir W. Herschel put forth his first catalogue of a thousand nebulæ. Fraunhofer's father was a glazier, and intending to bring him up to the same occupation, placed him early in the workshop, neglecting to give him proper school education. But when only eleven years of age he lost both his parents, and his guardian found him employment in the glass-grinding works of Weichselberger, at Munich, where he was to labour six years without pay, receiving instruction in return for his work, no other education being afforded him. Some time after the house where he was employed fell down (together with the adjoining one), and he was dug out from the ruins; it so happened that the Elector of Bavaria (Maximilian Joseph, who took the title of King four years afterwards, in 1805) was passing by, and he not only encouraged the poor boy's rescuers, but made him a present of 18 ducats. The accident, however, left a permanent weakness and probably shortened his life. With part of the money given him by the Elector he purchased a glass-grinding machine, and worked with it during his holidays, but without success. Ascertaining that this failure arose from his want of mathematical knowledge, he managed to devote his spare time (out of his employer's house) to the study of mathematics and optics, of which he acquired a competent knowledge even in those adverse circumstances. Finally, with the remainder of the Elector's present, he bought himself out of his last half-year with Weichselberger, and maintained himself by manufacturing visiting-cards, by which means he obtained more time for study, though the provision was far from adequate. Whilst thus engaged he became acquainted with von Utzschneider, who took him into the mathematical institute of Reichenbach, Utzschneider, and Liebherr as an optician. His first work was an object-glass for the Ofen Observatory, and from that time his progress was steady. In 1809 von Utzschneider, with his assistance and that of Reichenbach, erected the optical institute at Benedictbeurn. In 1811 Fraunhofer brought the glass-melting apparatus into complete order, and to him in 1818 was entrusted the direction of the whole. "His optical discoveries," says Schumacher, "are too well known to require mention here"; and I may say the same. His papers for the Bavarian *Gesellschaft der Wissenschaften* were afterwards translated by himself into French; his theory of haloes and parhelia appeared also amongst Schumacher's *Astronomische Abhandlungen*, and he contributed several papers to the *Astronomische Nachrichten*. His telescopes were superior to any previously constructed; that for the Dorpat Observatory was the largest hitherto made. He had one of equal size in hand, and had begun one of 12 inches aperture, when death carried him off on the 7th of June, 1826, not long after having completed his thirty-ninth year. Though unmarried he received every care during his illness. His patron, Joseph von Utzschneider, survived until 1840; the optical firm started by him in 1809 passed, some time after his death, into the hands of Messrs. Merz.

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[*] The late Mr. Ranyard thought that he might have seen what Wollaston saw, but not attributing importance to it did not mention the fact.

[†] Kurzer Umriss der Lebens-Geschichte des Herrn Dr. Joseph von Fraunhofer (Munich, 1826).

Yours faithfully,

Blackheath, 1902, Jan. 1. W. T. Lynn.

OBSERVATORIES.

[Continued from p. 65.]

Hamburg. *R. Schorr.*—The meridian work included stars in zone +80° and 81°. The equatorials were used for observation of comets. Time service. Meteorology.

HEIDELBERG.. *W. Valentiner.*—Usual meridian observation and the Sun on 25 days, Eros on 19 evenings. The 12-inch refractor was used for observation of Comets 1900 *b*, 1900 *c*, and for positions of Eros. Time service.

HEIDELBERG. *Max Wolf.*—This is the astrophysical portion of the Grand Ducal Observatory. Photography of small planets and nebulæ. Photometry of Variables.

JENA. *Otto Knopf.*—Comets and small planets. Meteorology.

KALOCSA. *J. Fenyi.*—Continuous observation of the Sun as in former years. The whole limb was examined on 143 occasions and partly on 16 occasions. Seventeen prominences of at least 100″ in height were noted. On June 1 the surface was disturbed by an eruption, and in the short space of 15 minutes a prominence 431″ high was produced, which soon dispersed: a similar extraordinarily short eruption occurred at the last minimum in 1888. Sun-spots were looked for on 197 days, on 88 of which the Sun was without spots, 36 days only one spot.

KASAN. *D. Dubiago.*—Very little astronomical work, as the instruments have not been fully installed in the new buildings.

KIEL. *P. Harzer.*—Dr. Ristenpart having been transferred to the Royal Academy of Sciences at Berlin, his place has been filled by Dr. Ebert, formerly of the Paris Observatory. The building for the new Transit-Circle is complete.

KIEL. *H. Kreutz.*—"Centralstelle für astronomische Telegramme," and origin of the *Astronomische Nachrichten.* Computation of small planet-orbits.

KÖNIGSBERG. *H. Struve.*—Comparison-stars for Eros, measures of double stars with the 13-inch refractor, also positions of Eros. The 3-inch transit was used on 50 evenings for pairs of stars in accordance with the Horrebow method.

LEIPZIG. *H. Bruns.*—The heliometer was in use for parallax observations of σ Draconis : for measuring the Hyades group and the diameter of Venus. Dr. Hayn employed the refractor in measuring positions of Lunar Craters. Time service.

MILAN. *G. Celoria.*—Prof. Schiaparelli resigned the Directorship in 1900 November, until which date he used the 18-inch for double-star work. The 8-inch was also used for double-star work. Geodetic work. Meteorology.

MÜNCHEN. *H. Seeliger.*—Herr Oertel made extensive experiments on the effect of direction in observing with the Repsold Meridian Circle. Using a reversion prism he finds $D-R = -0^s\cdot009$. The 10½-inch was employed by Dr. Villiger in observations of Comet Giacobini, measures of Saturn's system, and estimates of magnitude of Nova Aurigæ. Geodetic work.

O'Gyalla. *Baron B. Harkányi.*—Variable stars; Sun's surface on 206 days, on 152 of which the Sun was free from spots. Meteors. Time service.

Potsdam. *H. C. Vogel* (Astrophysical).—Dr. Hartmann took 132 spectra with spectrograph III. for purposes of adjustment, and 104 of stars and 46 spectra of faint stars with spectrograph I. These researches are in *Ast. Nach.* 3702–4. Dr. Hartmann confirms Prof. Campbell's observations of Polaris. An immense amount of other spectro-photographic work was done. Prof. Müller was engaged, as in former years, on photometric work. Prof. Kempf took 244 photographs of the Sun. In connection with the Astrographic Chart work 17 plates containing 5000 stars were measured; the reduction to show rectangular co-ordinates up to April 1900 was completed by Dr. Ludendorff.

PUBLICATIONS.

The Stars: a Study of the Universe *.—One has a little hesitation in beginning a notice of a book with the remark that it is "epoch-marking," for the epithet seems so trite; but nevertheless it exactly fits the book now before us. Of the past it may be said with very little limitation that all astronomy of position has been devoted to the determination of the geometry of the solar system. There are indications of many kinds that the astronomy of the future will have for its aim the determination of the geometry of the Universe. The data at present at hand for this, which is being added to day by day, are the enumeration of the visible stars and the tabulation of their numbers according to brightness, the measurement and classification of their parallaxes, and the evaluation of proper motion both in the line of sight and at right angles to that line. It is such information as this that Prof. Newcomb has collected in his book and discussed, it can scarcely be said with any definite conclusion, for the subject is as yet hardly ripe for that, but he indicates, in his usual masterly manner, the lines on which such researches should proceed.

To put the matter more definitely, we will turn to Chapter XIV., which has for its title "The Structure of the Heavens." Here the author states the answers to the problem of the Universe which have been supplied purely by the exercise of reasoning thought. It is said that the Universe cannot be unlimited in extent if light is never lost in its passage to any distance, however great, for if it were, and stars were distributed with equal density throughout unlimited space, the whole night sky would be as bright as day; therefore, since Physics teaches that light is never lost, it must be concluded that the Universe is limited. This argument depends, however, on the hypothesis that the stars are distributed impartially throughout space, and there is a loophole from the conclusion by supposing an arrangement such as that of Lambert, an eminent mathematician of the latter part of the eighteenth century, who assumed a number of planetary systems like our own, which in turn formed part of other systems, that a number of such larger systems form the Galaxy, or all the stars which we can see with our telescopes, and that the distant clusters are far-off galaxies. This construction is not open to the objection above stated, but modern observations seem to show it to be impossible. So far then as mental conception goes, the Universe must for the moment be accepted as limited, and the problem for solution remains, What is its shape?

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* By Simon Newcomb. London: John Murray, 1901. Price 6*s*.

The answer to this is to some extent supplied in the next chapter. The "grindstone" theory of Herschel and Struve, which supposes the solar system to be at about the centre of a multitude of stars arranged in the form of this object, so that looking out through the circumference, we look through many stars and see a picture such as the Milky Way, whereas in the directions at right angles to the plane of the sides we see comparatively few stars, seems to supply a convenient working hypothesis. This is easily put to the test by examination of the figures of Seeliger, who divided the sky into zones by small circles parallel to the Milky Way, and counted the stars down to magnitude 9·0 in each. The density per square degree in the nine zones, of which the central one is the Milky Way, and the first and last those at its poles, are given by the following figures:—2·78, 3·03, 3·54, 5·32, 8·17, 6·07, 3·71, 3·21, 3·14, which tend to show that the Universe is flattened between the galactic poles.

If it were as easy to determine the distance of a star as its direction, the problem of the actual distribution of stars in space would be solved. There are comparatively few stars whose parallax has been determined, but Prof. Newcomb proceeds to use this scanty material to find a numerical limit to the space occupied by the stars which are within our telescopic range. Suppose, says he, a sphere having the Sun as centre, and a radius R, equal, in round numbers, to 200,000 radii of the Earth's orbit, then a concentric sphere with radius 2R, and others with radii 3R, 4R.... The parallax of a star on the surface of the first sphere would be about 1″, on the surface of the 2nd, 3rd, 4th.... half a second, one third, one fourth, and so on. Counting up the parallaxes actually known, no star is found to be within the innermost sphere except our Sun, there is one within the second, four within the third, 28 within the fifth, and, if the volume of the first sphere be taken as unity, it will be found within certain limits of accuracy that there is one star to every eight units of space. Taking this ratio to hold universally, if the number of visible stars is 125 millions, which is probably an outside limit, they would all be contained in a sphere of radius 1000 R, or, in other words, no star has a parallax of 0''·001. This, as set down there, may seem arguing in a rather loose way, but it is all that can be done at present with the material at hand. Prof. Newcomb strengthens his argument by showing that the axiom of one star per eight units of space follows from consideration of determined proper motions, but to learn how this is done the reader must refer to the book itself.

We have given these few brief facts from the later chapters, since they evidently show the final aim of the book as they do of astronomical research. The early chapters may be considered as forming an introduction to this far-reaching investigation. They contain excellent treatises on schemes of star-magnitude, on star-spectra, on variable stars, and other such subjects, as well as a memoir on the names of the constellations, and a history of star-catalogues from Al-Sufi to the present day. The author in his preface almost apologizes for the book, for he explains what an immense amount of research and work is required to write a book of this kind, and says that he feels he has failed to surmount the difficulties thus arising in a way satisfactory to himself. Prof. Newcomb may be assured that no one but himself could have written this book, and that all astronomers will be grateful to him for having shown them the direction in which their future work must lie.

H. P. H.

MEASURES OF 900 DOUBLE AND MULTIPLE STARS *. — This volume is a welcome addition to our measures of double stars, its value being enhanced by the early publication. Prof. Doolittle used the 18-inch refractor of the Flower Observatory, and is grieved that the class of star is not that which he had set himself to observe. In consequence of the unfavourable weather, the volume contains many wide pairs which he otherwise would not have observed. We naturally sympathize with him, but in the present state of double-star work this turns out somewhat a gain. These stars must be observed, and of course ought to be measured by possessors of small instrumental power; but an idea has taken root that small refractors are of no use for double-star work, to which the largest refractors in the world are devoted. We are always pointing out the fallacy of this; there is abundant work for every class of instrument. As it is, workers with large instruments feel they must devote some time to measuring the wider pairs or they would become totally neglected. In the preparation of the present volume Prof. Doolittle has unconsciously helped to deter beginners by the perhaps too careful method of showing the separate results. In the class of star here observed, it seems superfluous to give the fraction of year for *each separate observation* to the third place of decimals, also the first decimal in angle would suffice. For example:—

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* 'Measures of 900 Double and Multiple Stars made with the 18-inch Refractor of the Flower Observatory.' Publications of the University of Pennsylvania, Astronomical Series, vol. i. part iii. 1901.

Σ 3116.

1898·096	22°·78	4″·37
1898·099	23 ·17	4 ·16
1898·115	23 ·92	4 ·31
1898·103	23 ·29	4 ·28

would do just as well if the 6, 9, 5, 3—8, 7, 2, 9 were left out, particularly as there follows

“Probably fixed.”

1831·16.... 19°·20 4″·48 Σ 4 nights.

Also the approximate catalogue-places might be given for 1900. There are very few misprints, the only one worth noting being the catalogue-name of Σ 80, which should be Piazzi 0^{h}·251, not 251 Piscium. A little more information might perhaps have been given concerning H 1968 on p. 13, which, being peculiarly a case of proper motion, should have its catalogue-name “Lalande 593” attached, the references to “Lick, vol. ii. p. 17, and *M. N.* 1894,” might have been included, and at the bottom of the column Sir J. Herschel's observation, 20″ ± 1*n*, is a little puzzling. T. L.

Pleasures of the Telescope *.—Most of this volume appeared first in *Popular Science Monthly*. The author, Garrett P. Serviss, sets out with the idea that an amateur is possessed of three refractors, a 3-inch, a 4-inch, and a 5-inch, and he describes in a most engaging way the objects which each instrument will show to advantage. Of course, an individual need not have three refractors; the idea is introduced as the readiest way of reaching all classes. A very practical and useful chapter on the principles and qualities of refractors introduces the book, and a theoretical chapter on “Planets among the Stars” closes it. The book is furnished with very clear maps inserted in the places where the letterpress requires them, not all at the end. In these maps only the double stars, variable stars, and nebulæ mentioned in the book are given, thus avoiding confusion. A good book for beginners.

NOTES.

Minor Planet Notes.—Dr. Bauschinger has celebrated the centenary of the discovery of Ceres by the publication of an interesting pamphlet (from the Berlin Rechen-Institut) containing exhaustive statistics concerning all the planets known by the end of 1900. We summarize the principal conclusions:—

(1) The ascending nodes show a distinct tendency to congregate in the neighbourhood of Jupiter's ascending node.

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* ‘Pleasures of the Telescope.’ Garrett P. Serviss. Published in London by Hirschfield Bros., 13 Furnival Street, Holborn, E.C., 1901.

Newcomb had deduced from theory that this was to be expected.

(2) 315 planets have inclination under 10°, 133 between 10° and 20°, 29 between 20° and 30°, 1 (Pallas) over 30°.

(3) 302 planets have eccentric angle ϕ under 10°, 157 between 10° and 20°, 2 over 20°. There is a slight tendency for large eccentricity and large inclination to go together.

(4) There is a very distinct tendency for the longitudes of perihelion to congregate near the longitude of Jupiter's perihelion. It is partly owing to this that September has far the highest record of discoveries. For the direction of Jupiter's perihelion is in opposition about that time.

(5) The next classification is by mean distance from the Sun. The most distinct gaps are at mean daily motion 900" and 600" (that is, three times and twice Jupiter's mean daily motion). These gaps are considered as dividing the family into three groups:

I.	The Mars-group,	μ between	2015" and 900"...	108	planets.
II.	The chief-group,	,,	900 ,, 600 ...	337	,,
III.	The Jupiter-group,	,,	600 ,, 400 ...	18	,,

A table is given of the other less strongly-marked gaps.

(6) Then follows a very interesting section dealing with the brightness and probable dimensions of the planets. The statistics make it probable that there are few undiscovered planets which are brighter than 10½ mag. at mean opposition. But we do not seem to be anywhere near exhausting the fainter ones.

Dr. Bauschinger assumes a mean albedo of 0·24, halfway between Mercury (0·19) and Mars (0·29). He deduces the following formula for the radius in kilometres ρ:—

$$\log \rho = 3{\cdot}3135 + \log[a(a-1)] - \frac{m_0}{5},$$

m_0 being the magnitude at mean opposition.

Using this formula, 202 planets have radii under 40 kilometres, 201 between 40 and 80 km., 41 between 80 and 120 km., 12 greater than 120 km.

The following are the radii in kilometres of the 12 largest minor planets as given by the formula, also of Eros:—

	km.		km.
(1) Ceres	386	(15) Eunomia	148
(2) Pallas	292	(16) Psyche	139
(3) Juno	146	(22) Kalliope......	126
(4) Vesta	417	(29) Amphitrite ..	143
(7) Iris	124	(349) Dembowska ..	128
(9) Metis	124	(433) Eros	16
(10) Hygiea	173		

The 12 largest ones form ⅔ of the volume of the entire family, and as it is probable that those still to be discovered are nearly all small, he estimates the combined volume as that of a sphere of 660 km. radius. This would have a volume $\frac{1}{900}$ of the Earth; and if we assume a mean density $\frac{2}{3}$ that of the Earth, the mass would be $\frac{1}{1350}$ of the Earth, or $\frac{1}{17}$ of the Moon.

If, instead of the mean albedo 0·24, we use 0·44, which is deduced from Prof. Barnard's micrometric measures of Ceres, Pallas, Juno, Vesta, we must multiply the radii given by Dr. Bauschinger's formula by the factor $\sqrt{\frac{6}{11}}$, or 0·7386. This would reduce the total mass to $\frac{1}{40}$ of that of the Moon.

Such estimates are, of course, not very reliable, for there may be many bodies of considerable (relative) size near the orbit of Jupiter, and invisible owing to their great distance. However, it seems pretty clear that, on any probable supposition, the mass of the entire family is considerably less than that of the Moon.

A. C. D. C.

OBITUARY.—JOHN BRETT, A.R.A. With regret we have to announce the death of Mr. John Brett, which happened early in January. To most persons the name will be familiar as that of a famous artist rather than that of an astronomer, but at one time he was a fairly regular attendant at the meetings of the Royal Astronomical Society, which he frequently enlivened by apt remarks, and contributed several valuable papers to the *Monthly Notices.* Mr. Brett went to Sicily with an expedition under Mr. Lockyer's direction to observe the total solar eclipse of 1870 December, when he was favoured with good weather, and was able to furnish an interesting report of his observations. He was elected a Fellow of the Society in June 1871, and shortly after contributed various papers, mostly of an optical nature, as might be expected from an artist. In one he appears to suggest that the luminous halo on which the Sun seems to be projected under normal conditions might be actually the corona; in another he sought for an explanation of the ring of light seen round the planet's limb on the occasion of the transit of Venus 1874, and suggested that if Venus had a translucent envelope of great density as well as a burnished reflecting surface, the appearance would be accounted for. He observed the partial eclipse of the Sun of 1874 October from Guernsey, and contributed a paper on his observations of this, making a suggestion, but with some reservation, that the existence of an atmosphere in the lunar valleys would account for some phenomena then seen. Other papers by him of a similar nature will be found in the *Monthly Notices.* Of his artistic career this is not the place to write in detail, but his pictures of sea and coast scenery are well known, more than one of his works having found a place in the Chantrey Collection at South Kensington. He died at his house, Daisyfield, Putney Heath, on January 7, aged 70.

THE DISTANCE OF NOVA PERSEI.—A valuable statement of Prof. Kapteyn's suggestion about the nebulosity round Nova Persei will be found in the report of the R. A. S. Meeting in this number, and the discussion which took place thereon seems to embody all the opinions on the subject. The important thing now seems to be to know the parallax of the star. Several attempts have been made at this as yet without any trustworthy result. M. Osten Bergstrand, of the Upsala Observatory, published last October (*A. N.* 3748) some measures of the distance of the Nova from neighbouring stars in March and September, which gave the unlooked-for result that its Right Ascension had increased between those days, whereas, if its parallax had been smaller than that of the comparison-stars, the R.A. should have diminished; the probability is, therefore, that this displacement is due to a proper motion of the Nova. Other persons have made similar measures, but no definitive value of the parallax has yet been published, no doubt after this month, when the star will have completed two parallax periods, something will be forthcoming.

Sir Charles Todd.—On December 5th last the heads of various departments connected with the Postal and Telegraph Service of South Australia met to congratulate Sir Charles Todd, the Deputy Postmaster-General, on the occasion of his Diamond Jubilee, he having completed his sixtieth year of service under the Imperial Government. We are careful to write "Imperial," because in these days, when the connections between Great Britain and her colonies are being emphasized, it is worth pointing out that many of the Staff of the Colonial Observatories are home-grown, and Greenwich is proud to think that Sir Charles Todd, the Director of the Adelaide Observatory and a leading official of the Post Office, is one of her sons. Sir Charles never forgets that fact, and in his speech on this occasion recalled the day in 1841, which, although in December, was the longest in his life, when he sat on an office-stool for the first time from 8 o'clock till 4 in the afternoon in Greenwich Observatory. His after career may be briefly sketched. After a few years computing at the Royal Observatory, he went as an observer to Cambridge for a short time, and then returned to Greenwich, where the chronograph which had recently been introduced for recording transits was put under his care; and as he showed himself very apt in electrical matters, when a man was enquired for to take charge of the newly established telegraph at Adelaide, Sir George Airy recommended Todd. How successfully he managed this work, and not only initiated the making of a telegraph-line across the Australian Continent, but also carried it out almost with his own hands, is well known. The Adelaide Observatory was afterwards given into his charge; and now in his 76th year he remains well and hearty, quite competent to carry on both his offices. No wonder his son said, in returning thanks for the family, that "with such a father they might be excused if they felt a pride in his career."

A SUGGESTED CAUSE OF COMETS' TAILS AND THE AURORA.—In the January number of the *Popular Science Monthly* there is a fascinating article by Prof. John Cox, of McGill University, Montreal, giving the gist of a theory lately put forward by the Swedish physicist Arrhenius, which promises to explain several things for which a definite cause has not yet been assigned. Prof. J. J. Thomson has shown that the corpuscles given off by the negative electrode in a Crookes' tube which form the cathode rays are of inconceivable minuteness—of about one-thousandth the size of a hydrogen atom—and it is also known that these corpuscles are given off by hot bodies such as glowing metals. Now the extreme minuteness of these particles makes it possible for them to be propelled by the Sun's light, for their surface-area is large compared with their mass, and hence the explanation.

In the case of comets, as they pass perihelion the Sun's heat sets up an action in the mass of which they are composed, so that they give off particles of different degrees of smallness, the larger of which are drawn back into the comet, but the smaller are driven radially into space by the Sun's light, and so form a tail. Another result of this theory of light-driven corpuscles is that the Sun is continually giving off into space electrically-charged particles, some of which reach the Earth, which might be the cause of various observed phenomena. At times of great solar activity more corpuscles reach the Earth, and, moreover, as they are propelled in the plane of the Sun's equator, their effect will be greater at the times of the year when the Earth is in that plane, which would account for the excess of Aurora Borealis seen in March and September. Arrhenius' theory seems to have the support of Prof. J. J. Thomson, and Prof. Cox's popular account of it, which explains in some detail how it may supply reasons for the Sun's corona, zodiacal light, and the Gegenschein, should be read, as we may hear more of this matter.

THE DIAMETERS OF JUPITER'S SATELLITES.—Prof. T. J. See, in *A. N.* 3764, gives another paper in connection with his researches on the diameters of planets and satellites. In this paper he gives his own measures of the satellites of Jupiter and Saturn made with the 26-inch Washington refractor, both at night and in the daytime just after sunset. From these he deduces the following corrections for irradiation:—

J. I.	0″·25	J. III.	0″·34
J. II.	0·23	J. IV.	0·27

and taking the mean of several observers since W. Struve he obtains:—

Satellite.	Observed diameter.	Corrected for irradiation.	Kilometres.
J. I.	1″·08	0″·88	3318
J. II.	0·96	0·76	2865
J. III.	1·58	1·28	4826
J. IV.	1·44	1·24	4675

"These values of irradiation are considerably smaller than the irradiation for Jupiter ($0''.75$). When projected on the planet's disc in transit all the satellites, as is well known, appear fainter than the body of Jupiter, except Satellite II., which appears as a white spot, and is thus brighter, area for area. Mr. E. J. Spitta's albedoes—0·656, 0·715, 0·405, 0·266, that of Jupiter being 0·624—conform accurately to my own estimates of the relative lustre of these several objects. But while Satellites I. and II. are slightly brighter, area for area, than the disc of Jupiter, their irradiations when off his disc nevertheless are smaller, on account of the great increase in the area of the satellite discs due to irradiation."

THE Lalande Prize of the French Academy of Sciences has been awarded to Dr. Thorne, Director of the Cordoba Observatory, for his Catalogue of Southern Stars from 22° to 52° South Declination, and for undertaking the Zone of the Astrographic Chart $-22°$ to $-31°$, which had been resigned by the La Plata Observatory.

MR. MEE's card almanac, "The Heavens at a Glance," 1902, is a useful adjunct to the astronomer's study-table. It gives, in a concise form, astronomical information that one often wants in a hurry, and saves hunting up a reference-book. It gives this year, for the first time, two small maps showing the northern and southern stars visible from Great Britain.

THE death is announced of Sir James Timmins Chance, a member of the well-known firm of glass manufacturers of Birmingham. Sixty years ago, when an undergraduate at Cambridge, he invented an ingenious process for grinding and polishing glass, which has not yet been superseded.

IT is stated that the Göttingen Academy of Sciences will maintain, at its own expense, during the period of the special international magnetic work, a magnetic and meteorological observatory near Apia, in the Samoan Islands, of which Mr. A. Nippoldt, of the Potsdam Observatory, will be in charge.

A MEDALLION bust of Sir George Airy has been placed, by his daughters, on the north-east wall of the Greenwich Parish Church, St. Alfege.

THE Annual General Meeting of the Royal Astronomical Society will be on Friday, February 14, at 3 o'clock in the afternoon. The next Meeting of the British Astronomical Association will be on Wednesday, February 26.

FROM AN OXFORD NOTE-BOOK.

THE most exasperating part of it all was, perhaps, the fact that if, instead of going away off into the interior to search for good weather, we had simply gone ashore from the gunboat that carried us to Sumatra, and put up our instruments on the sea-shore or near it, we should have had successful observations, for the ship's people had a fine view of the eclipse, with nearly twenty seconds longer duration of totality. But this was something that could not be foreseen, for the meteorological reports had made Solok the most favourable of all the stations . . . . . . One of the most unpromising places in Sumatra, according to the meteorological reports, was Padang-Panjang. It rained there all the time, and there was certainly no chance for observations at that point, so astronomers passed it by, *though it was more convenient than any of the other stations in the interior.* There was a story told of a Dutchman who lived at Padang-Panjang, who was very anxious to see the eclipse, but he had no faith in Padang-Panjang, and so he came down to Solok to get a view of the corona. As chance would have it, it was splendidly clear during totality at Padang-Panjang.

These words are extracted verbatim from Prof. Barnard's report on his expedition to observe the eclipse at Solok, in Sumatra (*Popular Astronomy*, No. 90), except that I have italicized a few words. We could not have a better example of the futility of being guided by meteorological forecasts *when there are other considerations of importance* (hence the italics). Among several stations equally convenient and with equal duration of totality, it would, of course, be foolish to choose that with a bad meteorological record in preference to that with a good. Success might attend such a choice, but would prove nothing. Indeed it will appear from the experience of the last sixteen years, as recounted below, that in this particular period success would have *invariably* attended a deliberate choice of the station with a poor meteorological record. But I certainly should not advocate doing this on principle. What I do think we may legitimately infer and act upon (and I have urged this for some years past in spite of one or two wise head-shakings) is that all other considerations should take precedence of meteorological promises. These last represent a *very* small possible gain, which has been magnified out of all proportion to its merits; and it is really a safer course to neglect it altogether. We have had some "fat" years for eclipses, and some "lean" ones are in prospect. Before our experience and its lessons are forgotten let us see what it amounts to.

1901. *Sumatra and Mauritius.* See extracts above.

1900. *Tripoli, Algiers, Spain, America.* Fine everywhere.

1898. *India.* Fine everywhere.

1896. *Norway*: cloudy. *Japan*: cloudy at Esashi and Akkeshi, which were selected in preference to Kushiro on the strength of meteorological reports. Fine at Kushiro. *Nova Zembla* (passed over by the official expeditions, mainly on the strength of meteorological reports; and occupied by Sir George Baden-Powell as an afterthought): fine.

1893. *Brazil and S. Africa.* Fine both stations, although forecasts were poor.

1889 (December). In *S. Africa* the meteorological reports of three years previous predicted *certain* fine weather; but it was cloudy for several days near the eclipse. In *Cayenne*, where the chances were considered poor, it was fine, and photographs were obtained, at the cost of Father Perry's life.

1889 (January). *America.* Fine everywhere (?).

1887. *Russia.* Cloudy everywhere.

1886. *West Indies.* The British expedition was split up into five parties, which scattered to avoid complete failure if possible. Mr. (now Sir Norman) Lockyer, who was in charge of one of these, carefully studied the meteorological conditions on the spot for some days; and in consequence of his observations determined to move to another locality with better promise. He was the only one who failed to see the eclipse; and Mr. W. H. Pickering, who arrived at the last moment and occupied the deserted station, had perhaps the best weather of all of us.

This is the extent of my personal experience, but it seems to me tolerably convincing. In not one single instance has the payment of attention to meteorological promises been rewarded; indeed it has been most cruelly and maliciously punished, where any notice has been taken of it. Either it has been uniformly fine or uniformly cloudy (and these possibilities must always be remembered: in these cases it does not matter where you go), or the choice between a good and bad station has resulted in disaster. Had we not better give up entirely the attempt to select stations in this way? We should save a good deal of labour and expense in collecting information, and the exasperation of failure in the end.

WE are under another obligation to Dr. Chandler for discovering new merits in Bradley's work (*Ast. J.* 513). It would appear that Bradley was within measurable distance of adding the discovery of the variation of latitude to those of aberration and nutation: indeed he got so far as to see that one year's results did not agree with the next, of suspecting his instrument and finding it correct when tested. But of course the discrepancy was of a much smaller order than the others, and it is not surprising that he failed to completely anticipate the outcome of another 150 years' observations; it is only wonderful that he should have foreshadowed them, however faintly. To the gratitude of Greenwich for this last revelation of its past achievements, I will add a tribute from Oxford. Bradley was an Oxford man; graduated at Balliol in the regular way; and came back to be Savilian Professor of Astronomy, an office which he held till his death (for the last 20 years along with that of Astronomer Royal). There was no observatory at Oxford in those days, and he made his earlier observations at Kew and Wansted, and the later ones at Greenwich. So that Oxford can only claim a small part of the glory: but still I put in the claim for what it is worth.

THE question has been put to me by Dr. Chandler, What is the correct spelling of Wansted? Bradley spells it Wansted, but now-a-days it more often appears as Wanstead. I replied that I did not know, but would ask the best authority I knew of, viz., Dr. J. A. H. Murray, who is making the big Oxford dictionary: and Dr. Murray very kindly sent a full explanation about spelling generally, which seemed so interesting that I asked his permission to print it. At first he demurred, saying that much of it must be too well-known; but on my assuring him that though I had no doubt I *ought* to know it, it was almost entirely new to me and might be new therefore to others, he kindly consented. But I have given this personal explanation in order that the responsibility for publication may rest on the proper shoulders.

Language, being a function of living beings, is in its form continually changing, so that no one speaks quite the same as his grandfather, much less the same as more remote ancestors. The *written* form follows these changes laggingly and haltingly, because, while nobody has heard his great-great-grandfather *speak*, everybody knows or may know how men *wrote*, two, four, six, or eight hundred years ago, and has thus a pattern to adhere to which he has not in speech itself: although the phonograph may, in course of time, preserve the actual speech of past generations. Now changes in spelling, as well as changes in speech, come gradually; there are always current two or more forms, older and newer, of *spoken* words, and often of *written* words. The word *instead* has had, since the 13th century, the following forms:—

14th–15th c., *in stede*; 15th c., *in sted*; 16th, *in steede*; 16th–17th, *in steed*; 17th–18th, *insted*; 18th–19th, *instead.*

The second syllable of *Wanste(a)d* is identical with the second of *instead*, viz., the word *stead*, originally *stede*, meaning "place." This is now commonly spelt *stead*: and most names of places like Hampstead, Newstead, Banstead, &c. are now commonly spelt in the same way. But it cannot be said that *-stead* is more "correct" than *-sted*: that is, if "correct" means representing the sound in the most exact way possible with the means at our disposal, since *-sted* can have only one sound, while *-stead* may rime (sic !) with *bread, dead*, or with *bead, plead*; the unambiguous *-sted* therefore would be a more "correct" spelling: but the ambiguous *-stead* is at present usual; and as the purpose of language is not primarily to be "correct," but primarily to be intelligible, and secondarily to be "correct," correctness has generally to be sacrificed to intelligibility: *i.e.*, to doing what at the time is customary. The notion that there is a "proper" spelling of a word is one of recent origin, created mainly by printers' readers for saving time and expense in correcting proof, and cherished by school-inspectors as one of the simplest means of plucking examinees: in a natural state there are as many varieties of spelling as there are of pronunciation. If you wish to be minute in the spelling of Wanste(a)d you may say, "the place now, in the English postal directory, spelt *Wanstead*, but in the 17th and earlier 18th century usually *Wansted*, in the country now spelt *England*, formerly (as still pronounced) *Ingland*, originally *Engla-land*."

Rumours have reached us lately that some of the German newspapers have not been altogether complimentary to our soldiers, or to us generally. It may help us to bear up under this affliction if we glance occasionally at what some German newspapers say of the heavenly bodies; and so I will reprint here an extract from the *Neue Hamburger Zeitung* of November 26 last:—

Die Finsternis war bedeutend stärker, als bei dem letzten Vorübergang des hellen Mondes vor der Sonne am 28. Mai 1900, nachmittags. Die wahre Ursache dieser ungewöhnlichen Finsternis ist der *Vorübergang eines Dunkelmondes vor der Sonne*, ein Ereignis, welches ziemlich häufig stattfindet, aber in unserem Klima mit seinem bedeckten Himmel selten beachtet wird. Dieser Weltkörper bewegt sich um die Erde in 14 Stunden 23 Minuten, aber von einem Zusammentreffen mit der Sonne bis zum nächsten verlaufen 14 Tage 13 Stunden 48 Minuten. Seine scheinbare Grösse ist derjenigen des hellen Mondes ziemlich gleich, aber da er der Erde viel näher steht, ist er thätsächlich bedeutend kleiner. Die Entfernung beträgt 250,000 Kilometer. Dieser Weltkörper ist von einer Atmosphäre umgeben, welche zwar das Licht der Sonne einsaugt, aber nicht wieder in den Weltraum hinausstrahlt; nur im Zustande elektrischer Erregung wird er uns als leuchtende Scheibe sichtbar. Eine ähnliche Verdunkelung hat sich auch 1891 am 17. April ereignet, worüber die Deutsche Seewarte damals auch berichtet hat. Es wäre sehr wünschenswert, wenn die geehrten Leser dieser Zeitung etwaige weitere Beobachtungen mitteilen wollten; vielleicht lässt sich dann auch feststellen, dass der Gang der Verdunkelung in Deutschland von West nach Ost erfolgt ist.

By his utterance with regard to Astronomy, to which attention was called a couple of months ago, Lord Rosebery seems to have drawn a fire of astronomical allusion. In a leader on the Chesterfield speech, the *Daily Telegraph* of January 8 has the following passage:—

> Astronomers, before the days of highly-developed telescopes, were apt to confound clusters of fixed stars with nebulæ, though there was nothing in common between them except their appearance when studied with low powers. The star of the cluster is a world in being; nebulæ—if the nebular hypothesis is true—are a world in the process of making. To which class does Lord Rosebery belong? Is he the most brilliant of a detached group of stars, or is he, so to speak, the nucleus of a new political body? That is what we do not know, and we do not gain much instruction from the lesser lights around him.

There is not much point in the metaphor, and it seems a fair supposition that Lord Rosebery would have escaped it entirely but for his attempt at dissuasion from astronomical studies. The Fates have apparently devised a suitable punishment for him: he is probably going to have a "sending" of these things, and it will be interesting to look out for them. Meanwhile there is a curious little utterance in a book called 'Four Leaved Clover,' by Maxwell Gray, which got into the note-book, and may as well be set down here as a minor contribution to the discussion. Lord Sharland gives the following advice to a person in grief:—

> Minds can't rest except relatively. Why don't you learn astronomy? I had a trouble something like this once, Marcia. So I threw myself into astronomy and the higher mathematics and recovered. That is to say, became able to bear it.

Is the following little rule, which I find jotted down on the same page of the note-book, well-known? There is nothing particularly new about it, of course:—

> To find distance of sea-horizon in miles: add half the height in feet and take the square root.

Mr. T. J. Moore, of Doncaster, draws attention to an opportunity of measuring the size of Venus during the next month, in an interesting letter:—

> I beg to call your attention to a little matter in relation to the planet Venus at the coming conjunction on Feb. 14. Eight years ago her position in the heavens was similar; and I well remember seeing her as a morning star on the morning of the day when she came into conjunction with the Sun. Having a declination some 8° or 9° north of the Sun she rose before him on that morning, although she did not arrive at conjunction until, I believe, the afternoon. However, the sky was beautifully clear at noon on the day in question, and I had no difficulty in picking her up with the "finder" of my 5-inch reflector, which is of 1-inch aperture (cut down by diaphragm to less than ¾-inch). Her thin crescent was quite conspicuous, but not to the extreme points. With the larger instrument the points of the cusps appeared so narrow that I felt that, had I had a double-image micrometer in my possession, I could easily have determined her diameter to within 1″, corresponding to about 0″·3 at mean distance; and since there is a discrepancy of more than 1″ in the results obtained by different observers using the most modern improvements, I think that those who have the instrumental means at their disposal should take advantage of this favourable opportunity.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 February 14.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

Mr. Whittaker read the Minutes of the last Annual Meeting, which were confirmed.

Mr. H. P. Hollis read the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1901, and had found and certified the same to be correct. The cash balance in hand on 1901 December 31 was £388 18*s.* 10*d.*, as compared with £549 12*s.* 10*d.* on 1901 January 1, and that during the year £340 12*s.* 6*d.* had been invested in the purchase of £400 Midland 2½ per cent. Debenture Stock. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in good condition; and they added a note to the effect that, in their opinion, it would be to the interest of the Society that the amount of the Composition-Fee paid in lieu of Annual Subscription should be increased.

Mr. Dyson read the list of names of Fellows and Associates who had died during the year, with some extracts from the obituary notices prepared by the Council. Following the name of the Patron of the Society, Her late Majesty Queen Victoria, the list was made up as follows:—Rev. J. Chadwick Bates, E. E. Bowen, John Brett, Rev. R. Crowe, Maj.-Gen. A. W. Drayson, T. P. Hartree, Lord Inverclyde, S. Wellesley Johnson, William Lethbridge, D. L. Lowson, Charles Meldrum, John Parnell, Sir Cuthbert E. Peek, B. Wood-Smith: and Associates, H. A. Rowland, T. H. Safford, Wilhelm Schur.

Mr. Whittaker read extracts from the Reports of Observatories which form part of the Council's Report.

Mr. Dyson read extracts from the notes on the progress of astronomy during the year, which dealt with the following subjects:—Meteoric Astronomy, Discovery of Minor Planets, the Planet Eros, Solar Activity, Photometry and Variable Stars, Double Stars, the Comets of 1901, the Revision of Taylor's Madras Catalogue, 1835, Total Solar Eclipses, Geodesy, and Latitude Variation.

The President then announced that the Council had awarded the Gold Medal of the Society to Prof. J. C. Kapteyn, of the University of Groningen, Holland, for his work in connection with the Cape Photographic Durchmusterung and for his researches on stellar distribution and parallax. In conclusion, addressing Prof. Kapteyn, he said:—"Professor Kapteyn: In placing in your hands this Gold Medal I wish specially to congratulate you upon the completion of the Cape Durchmusterung, and to express to you the hope that your life may long be spared, and that you may have health and strength to extract from this grand storehouse of your own formation many more of the great cosmical truths which it implicitly contains, and be enabled to enrich still further by your labours the important branch of our science to which you have devoted your life."

Professor Kapteyn. Mr. President, will you allow me to say a few sentences in answer to your kind words, and to thank the Society for the honour conferred upon me to-day. Apart from the satisfaction attending the study of the science of astronomy, which must appeal to all those present, I cannot imagine any greater satisfaction than that derived from the approbation of the most competent men. I can clearly see from the feeling of this Meeting that this Medal is not only a reward for work done. I can also clearly see that the award has another meaning, namely, encouragement to work in the future. I can assure you, Mr. President, and I can assure the Society, that apart from the satisfaction of studying science for its own sake, there can be no greater encouragement for me than to try and obtain your further approbation by continuing, with all the zeal that is in me, the work for which you so much honour me this day.

The President then invited Dr. T. D. Anderson to ascend the dais, and addressed him as follows:—"Dr. Anderson: It is a great pleasure to me to hand you on behalf of the Society the Jackson-Gwilt Medal in recognition of your discovery of both Nova Aurigæ and Nova Persei. Nova Aurigæ was discovered by you on 1892 February 1, when of the 4th magnitude, and but for your discovery it might have escaped observation. Nova Persei was discovered on February 22 of last year at 2.40 A.M., when of the 2·7 magnitude and low down in the sky. This early discovery of yours made it possible for Pickering to obtain its spectrum before its maximum was reached, the spectrum afterwards being quite different.

"It is no small matter to have discovered one of these Novas, but it is a veritable *tour de force* such as *a priori* would have seemed almost incredible to have discovered both, and I am delighted that we have the opportunity to congratulate you on your success and do honour to your astronomical zeal and intimate knowledge of the sky. Between your two discoveries several new stars had been found at Harvard, but all by photography.

"I ought, perhaps, to refer at greater length to the number of variable stars—no less than thirty-eight—that you have discovered; but on the present occasion I should prefer to restrict myself to the subject of the award, your wonderful discoveries of the two great Novas of our time."

Major Kingsley Foster. Mr. President, I have now to move that the Report of the Council be adopted, and that it be printed in the usual manner, together with the President's Address and the Report of the Auditors.

Mr. F. McClean. I shall be glad to second that.

The motion was unanimously adopted.

Prof. Turner. I have now the honour to move the resolution of which notice has been given, "That henceforth the Annual General Meeting of the Society be held at Five o'clock, and that Bye-law 45 be altered accordingly." I hope it is not necessary to make any very lengthy remarks in support of this resolution, which, to my mind, is only the outcome of our altering the hour of the evening Meeting from 8 o'clock to 5 o'clock. I should rather look for those who may have any reasons against the alteration to state those reasons fully and frankly before the Fellows assembled here. One objection which existed, and which I felt the full force of, in altering the hour of the evening Meeting, was that those present and voting at the Annual Meeting were not all those whose convenience was affected. That objection does not apply here, as the Fellows have assembled at the Annual Meeting simply to consult their own convenience, and it is simply for them to say whether it will be for their convenience to alter the hour of meeting from 3 o'clock to 5 o'clock. It is an obviously convenient arrangement, so far as memory is concerned, to have one hour uniformly throughout the year, instead of a different hour for the Annual Meeting, which, so far as I am aware, has no convenience attached to it. I think it is right to mention, though I lay no stress upon it, that at present the hour of meeting at 3 o'clock necessitates the holding of the Council Meeting which corresponds with this meeting a week earlier. It may be, if the hour is altered, the Council will change their Meeting to the same day as the Annual Meeting; and I might mention in my own case, coming from Oxford, it would be a distinct personal convenience to me. It will make one journey instead of two, and the same would be the case for several members of the Council. However, I do not think any stress should be laid upon that, in comparison with what may be for the general convenience.

The Astronomer Royal. I shall be glad to second the motion, but, as Prof. Turner has said, it is a question for the Fellows generally to decide. It seems to me that the Meeting to-day is very much fuller at this time (5 o'clock) than when I came in at 3 o'clock. I think it will be found more convenient to meet at 5 o'clock, but this is a question for the Fellows to decide themselves.

Captain Noble. As one of those who supported actively the suggestion to change the hour of our Meeting from 8 o'clock to 5 o'clock, I should certainly oppose this idea of altering the hour of our Anniversary Meeting. If we are going to have that Meeting at 5 o'clock, and if the proceedings are going to be protracted like this—for we are not through our work of to-day yet—I do not know at what time we shall get away on the nights of the Annual Meeting. On the night of the Ordinary Meeting, unless we have a great many papers, we do get away by or before 7 o'clock, but whether we should do so, assuming we alter the Annual Meeting hour, I think is extremely doubtful.

Mr. Knobel. It is rather surprising, Mr. President, that a motion to alter our custom of so many years should not have been supported by some arguments. The only argument Prof. Turner has advanced is that all the Meetings throughout the year would be at the same hour, and also that it would be a convenience to himself to come up from Oxford. It seems to me that what Capt. Noble has brought forward is a very serious point for consideration. At the present moment, supposing the 5 o'clock hour had been in force, it would have been 7.10 ere we had reached our present stage, and the business is not yet over. We have all got to go to dinner some time or other, and by the arrangement proposed we should not get away, perhaps, till 7.30. That is the position in which we should stand. We know perfectly well that there is a great deal of business to be done at the Annual Meeting. The reading of the Council's Report by the Secretaries has always been a real thing—never a farce—and I hope it will never be otherwise. It is most important that the Council's Report should be fully read to the Meeting. Then we have the President's Address, which is also a lengthy proceeding. We have often had discussions which have lasted very much longer than this discussion is going to last. The difficulty is that the Annual Meeting may last, say, for three hours. These are very strong arguments against making the Meeting later. If Professor Turner had made the more modified proposition of 4 o'clock it would have been much more agreeable; but as to 5 o'clock, I think it is a very ill-advised proposition, and I hope it will be entirely objected to.

The President. I think we have practically made up our minds as to the hour we prefer. There is not much to discuss about it, so I will proceed to take the vote.

Professor Turner. May I reply?

The President. Is it worth while?

A Fellow. I should like to ask the President what the effect has been of the change from 8 o'clock to 5 o'clock.

Professor Turner. I only wish to say, in response to the arguments that have been advanced, that in my opinion any meeting can be got over in two hours if there are only two hours to spare. If there are four hours to spare there is a tendency to take four hours over it. The reading of the report has always been a serious business, and I hope it will continue to be so; but there are degrees even in that. It is just a question of how many points are put in. It would take five hours to read the whole Report; but a selection is made to suit the convenience of the Meeting.

The President then put the motion to the Meeting, and after a show of hands declared it carried by 18 to 15 votes, remarking at the same time that he wished it had been adopted by a larger majority.

The President then appointed Mr. A. Fowler and Mr. Hardcastle to be Scrutineers of the Ballot, which was then taken, and the following Officers and Council were declared elected:—

President: J. W. L. Glaisher. *Vice-Presidents*: Sir William Abney, W. H. M. Christie (Astronomer Royal), E. B. Knobel, and H. F. Newall. *Treasurer*: W. H. Maw. *Secretaries*: F. W. Dyson and E. T. Whittaker. *Foreign Secretary*: Sir William Huggins. *Council*: Sir R. S. Ball, A. A. Common, A. M. W. Downing, Major E. H. Hills, Thomas Lewis, Frank McClean, Major P. A. MacMahon, A. A. Rambaut, G. M. Seabroke, E. J. Spitta, W. G. Thackeray, H. H. Turner.

It was proposed by *Mr. W. W. Bryant*, and seconded by *Mr. G. S. Criswick*, "That a vote of thanks be accorded to the Vice-Presidents and other Members of the Council who now retire, for their services in promoting the objects of the Society."

On the motion of *Col. Bigg-Wither*, seconded by *Major Kingsley Foster*, a vote of thanks was accorded to the Auditors of the Treasurer's accounts and the Scrutineers of the ballot.

The following gentlemen were elected Fellows of the Society:—

William Ambrose Kibbler, M.B., Ashby House, 3 Manor Road, Stamford Hill, N.

C. F. Sandberg, M.A. Oxon., Magdalen College School, and 6 Parks Road, Oxford.

Albert Walter, Royal Alfred Observatory, Mauritius.

Edward Fawcett White, Navigation Department, and Merchant Venturers' Technical College, Bristol.

The following Candidates were proposed for election as Fellows of the Society:—

George Herbert Bryant, Science Master, Hipperdale Schools, Cinderford, Gloucestershire (proposed by Albert Taylor).

Capt. G. R. Lenox-Conyngham, R.E., Survey of India, Mussoorie (proposed by Major Hills).

S. Alfred Mitchell, M.A., Ph.D., Tutor in Astronomy at Columbia University, New York City, U.S.A. (proposed by J. K. Rees).

James Rankine, 11 West Saville Road, Edinburgh (proposed by William Peck).

Thomas Thorp, Moss Bank, Whitefield, near Manchester (proposed by David G. Simpson).

William Boultbee Whall, Younger Brother of the Trinity House, Nautical Surveyor, Board of Trade (proposed by M. J. O'Sullivan).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fourth Meeting of the twelfth Session of the British Astronomical Association was held at Sion College, on Wednesday, the 29th January, 1902, *Mr. G. M. Seabroke*, President, in the Chair.

Mr. G. F. Chambers read a paper on "The Total Eclipse of 1905." He remarked, that as this eclipse would be visible within easy reach of England, it was desirable to gather the necessary information in good time. The Madrid Observatory had already published a very fine large-scale chart of the path of the eclipse across Spain. The time of the year, August 30, was not a favourable one for travelling in Spain, but British astronomers would do well to turn the opportunity to the best account, as their next chances of studying solar eclipse phenomena would only be as follows:—1912, April 17, when there would also be an eclipse total in Spain; 1914, August 21, total in Norway; and 1927, June 29, total in the north of England. Having regard to the accessibility of the central line and the comparatively long duration (3½ minutes) of the eclipse, this of 1905 might be regarded as the most important eclipse of the first quarter of the 20th century as concerns Western Europe. The weather prospects for the end of August in Spain were very favourable. Mr. Chambers then described in some detail the suitability of various towns for the different kinds of observational work, noting those with good or fair hotel accommodation.

Mr. A. C. D. Crommelin said he was sure they were all very much obliged to Mr. Chambers for the considerable geographical information he had furnished them with, with regard to the coming eclipse. In the recent Spanish and Algerian eclipse the duration of totality was not so great as they would have liked; but in 1905, the duration would be three times as long. It appeared to him that the Balearic Islands would be a very suitable station, easy of access *viâ* Marseilles and with not so torrid a climate as some parts of Spain. The eclipse might also be seen in Northern Canada, Tunis, and Egypt.

The President expressed his thanks to Mr. Chambers for having brought so many interesting geographical particulars of the coming eclipse before the Association so soon.

Mr. W. E. Besley contributed a paper on the Progress of the Leonid Shower, 1901. Although the shower was not at any time such a striking spectacle as were those of 1866 and 1868, yet meteors appeared in much larger numbers than in 1900. Mr. Besley gave a tabular summary of an effort to obtain approximately the average hourly rates of true Leonids appearing to observers watching the sky without interruption on two nights, Nov. 14 and 15, 1901.

Some photographs of the Singkep Mechanical Commutator sent by *Mr. David P. Todd*, of Observatory House, Amherst, Massachusetts, were then thrown upon the screen, and described at length by Mr. Crommelin, from notes by Mr. Todd.

Mr. Crommelin exhibited slides showing the nebulosity round Nova Persei, photographed by Prof. Ritchey at the Yerkes Observatory, on Sept. 20 and Nov. 13, 1901. He said, the remarkable thing about them was the extraordinarily rapid change between these dates—in particular, one well-marked tongue of nebula had moved over a minute of arc, which implied an almost inconceivable speed. The Nova was an enormous distance from us, at least 60 light-years, in which case the velocity of the tongue was 40,000 miles per second. They could scarcely imagine matter rushing through space at that rate, so Prof. Kapteyn had made the suggestion that the phenomenon was due to the light from the Nova reaching successively more and more distant points of the nebula, and so being reflected to us, and rendering the nebula visible. On this supposition the nebula would be expanding with the speed of light, and its distance would be 290 light-years, so that the outbreak took place in the reign of James I. The nebula was very faint and could not be seen by the eye, and could only be photographed with an exposure of several hours. Mr. Newall suggested that the actinic radiations from the Nova might effect some chemical change in the nebula and cause it to glow: this would imply the same distance for the Nova as Kapteyn's hypothesis.

The President observed that it might be as well to remember that, although they had only direct evidence of what appeared to be light going away from the central star, and passing through the nebula, and being reflected therefrom to us, still it was fair to assume that radiant energy generally was emitted from the star, and not simply that form of it which we call light. Hence the luminosity of the nebula might be sufficiently accounted for.

A letter from *Capt. P. B. Molesworth, R.E.*, of Trincomalee, Ceylon, to Mr. Maunder, giving his photographic experiences in connection with the annular eclipse, was then read to the Meeting.

Mr. W. F. Denning contributed a paper on "The Meteoric Shower of Halley's Comet." Dr. Smart, he said, in giving particulars of the next return of Halley's Comet in 1910, mentioned that, if the comet was accompanied with meteors, we might have a shower on or about May 17th, when the Earth reached that part of its orbit corresponding with the descending node of the comet. The Aquarid meteor shower (supposed to have derived its origin from Halley's Comet) had, however, always been seen either at the end of April or early in May, never so late as the middle of the latter month. The reason seemed to be that the Earth made her nearest approach to the comet's orbit about 12 days before nodal conjunction. Prof. A. S. Herschel gave the most probable date of the Halleyan meteoric shower as May 4, the position of the Earth being then distant about 5½ millions of miles from the cometary orbit, and the theoretical radiant was stated to be at $337^\circ + 0^\circ$, and the velocity of the meteors 41 miles per second. Lieut.-Col. Tupman was the first to discover this important shower of η Aquarids. It apparently furnished meteors between April 29 and May 10. From observations made in several years from 1876 to 1900, the mean radiant came out at α 336°·4, δ −2°·5.

Mr. Alexander Smith's slides of his observatory and telescope at Dalbeattie were then exhibited.

Papers on "The Distribution of Stars," by *Mr. Gavin J. Burns, B.Sc.*, and on "Some Southern Nebulæ," by *Mr. Hugh Wright* of Sydney, N.S.W., were then read, and the Meeting adjourned at 7 P.M. to February 26th.

THE ROYAL METEOROLOGICAL SOCIETY.

The usual Monthly Meeting was held on Wednesday, February 19th, at the Society's rooms, 70 Victoria Street Westminster, *Mr. W. H. Dines, B.A.*, President, in the chair.

Ten new Fellows were elected.

Mr. E. Mawley, F.R.H.S., submitted his report on the Phenological Observations for the year 1901. He showed that, as affecting vegetation, the weather was chiefly remarkable for the scanty rainfall during the growing period of the year. The deficiency was not confined to any part of the British Isles, but was more keenly felt in the English Counties than in either Scotland or Ireland. Wild plants came into flower very late, but not quite as late as in the previous phenological year, which was an exceptionally backward one. The swallow, cuckoo, and other spring migrants were, as a rule, rather behind their usual dates in reaching these Islands. The crops of wheat, barley, and oats were all more or less above average in Scotland and Ireland. On the other hand, in England, although there was a fair yield of wheat, that of barley and oats was very deficient. Hay proved everywhere a small crop, and especially so in the southern districts of England. Beans, peas, turnips, swedes, mangolds, and potatoes were all more or less under average in England, but either good or fairly good elsewhere. The yield of hops proved singularly abundant. Apples, pears, and plums were below average, especially apples, but the small fruits, as a rule, yielded well.

Taking farm and garden crops together, seldom has there been a less bountiful year.

The Constellations.

An Admiral of the British Navy once, in commenting on the division of the starry heavens into constellations, gave an illustration of the confusion which exists in modern atlases, and makes it a plea for judicious simplification, in the following words:—

> The boundaries of each asterism should be indelibly fixed, so that Aries should no longer have a horn in Pisces and a leg in Cetus; nor 13 Argûs pass through the Unicorn's flank into the Little Dog; 51 Camelopardi might be extracted from the eye of Aurigæ; the ribs of Aquarius released from 46 Capricorni, and numerous other vagaries of the kind corrected. Then again there is such a jumble of Greek, Roman, and Italian letters, that the profusion is quite confusing.

At another time he referred to some of the many amendments which had been proposed by others:—

> Ages before Julius Schiller had re-baptized the planets and re-formed the constellations by assigning scriptural names to them, our Venerable Bede had already enlisted the twelve Apostles, in order to displace the profane signs of the zodiac.

England's first Astronomer Royal, more than a century earlier than Admiral Smyth, was ever railing at Halley for the introduction of new constellations and new confusions among the old constellations in the southern hemisphere. He was especially bitter towards Halley for "mutilating" his catalogue, by altering the stellar nomenclature, when the pirated edition of it was issued in 1710-11. As Flamsteed also pointed out, even among those who claimed to follow Ptolemy exclusively in these configurations (Bayer, for instance), the figures were often reversed so that a star described by Ptolemy as in the right side of man or beast would appear on the maps as in the left side, and so on.

New constellations have been added from time to time by Hevelius, Bode, Lalande, and others, but these are usually to embrace the scattered stars between the ancient constellations and seldom displace or replace the Ptolemaic constellations. Yet modern astronomers have seldom been able to establish a permanent pre-emption claim on such starry spots, since the same assemblage of stars not infrequently has several modern appellations.

Often these are of such diverse character as to render conclusive the opinion that their configuration could usually not have suggested the outlines of the object assigned to them, however fervid the imagination of the zealous innovator. The lizard or stellion, the hand and sceptre of Justice, and the coat of arms of Frederick, for example, mark the same spot, according as the nomenclature may be that of Hevelius, of Royer, or of Bode. There have thus grown into our literature nearly a hundred constellations in excess of the forty enumerated by Ptolemy.

Not alone does this confusion exist regarding the constellations, but it extends into the nomenclature of the individual stars. For three thousand years or more the Chinese, Hindus, Greeks, Romans, and Arabs have assigned to the brighter stars descriptive or imaginative names. Many of these names have come down to us in the literature of science; and most of them (particularly those from the Arabic sources) have retained their places in many modern catalogues and atlases. It seems to be the tendency of contemporary economy in the publication of the majority of recent large catalogues, however, to ignore entirely the designation of a star's identity by its individual name, or even by its generic name, and to fix it entirely by its position in co-ordinates of right ascension and declination. These co-ordinates are themselves an innovation wholly supplanting the longitudes and latitudes of Tycho's and Flamsteed's day, while, at the present time, some are urging vigorously and with much show of reason the advisability of discarding the right ascension and declination for the rectangular co-ordinates suggested by economy in the reduction of photographic plates. This improvement will doubtless come in time, though probably never to the entire exclusion of the α and δ. With this change will likewise be more persistent the tendency to abandon constellation division and individual nomenclature for the few bright stars among the tens of thousands of fainter and nameless ones which are rapidly finding definitive places in catalogues of precision.

From Kepler's material cubes, octahedrons, &c., in which the transparent but solid spheres of the planets were enclosed according to his fertile imagination, and from the conclusions which he drew from the motions of his air-breathing Mars animated by the spark of life, ultimately came the inanimate, cold, analytic equations of Newton and Lagrange. The pace was a quicker one than that which marks the transition from the pictured heavens of the Chaldean shepherds and of the Arabian warriors, to the cold type of the Astronomische Gesellschaft Catalogue. But the former served its purpose well, though but the imagination of a single man: the division of the heavens into constellations, as a country into states, counties, and towns, has for ages also served a good purpose—not alone for one man, nor for one nation, but universally; so that now much of the myth, tradition, and fable of antiquity is in one way or another connected with or commemorated by the beasts and heroes and vessels which still snarl or hiss, hunt, or sail in that domain which knows no bounds—the blue sea of night, the snowy plain of day.

However assiduous, therefore, the astronomer may be in reducing a star's co-ordinates to its last residuum of refinement in the hundredths of seconds, however much of realism may enter into his mental make-up to the exclusion of the imaginative, however sober his reflections to the expulsion of life's humour, why should he not, nevertheless (yea, even by very reason of this constitutional defect), devote some portion of his time to acquainting himself with the romance and legends which adorn the ancient lore of his science, pictured for him in the heavens, and which will add a new charm to his nightly vigils, and in this matter (if no other) extend his knowledge of the classics beyond the limits of the ordinary collegiate curriculum.

The writer knows of no single book which gives at the same time so comprehensive, so complete, and so concise an enumeration of the names of the stars and constellations, and disentangles so well the many confusions, as one* which was published two years ago in New York.

As a work of reference it is invaluable, and as suggesting lines of thought for the further researches of philologist and antiquarian it is excellent. In the Introduction the author says:—

> Originality is not claimed for my book. Much of it has been gathered from widely scattered sources, brought together here for the first time in readily accessible form, although, doubtless, with errors and certainly with much omission, for while I have sought, as did Milton's *Il Penseroso*, to
>
> "sit and rightly spell
> Of every star that heav'n doth show,"
>
> yet in preparing my material I have seen, as Doctor Samuel Johnson wrote in the preface of his *Dictionary*,
>
> "That one enquiry only gave occasion to another, that book referred to book, that to search was not always to find, and that to find was not always to be informed."
>
> while to temper such criticism as may be bestowed upon my efforts, I quote again from the same source:
>
> "Dictionaries are like watches; the worst is better than none, and the best cannot be expected to go quite true."

So frank a confession would be more laudable if it were less misleading. Having been gathering notes on the same subject for the past decade during a wide range of reading, and by reason of an innate interest in this subject, the writer has through the past year derived much pleasure from a critical examination of this book. There are, indeed, some errors and some omissions, but, so far as the writer's researches go, they are very few indeed in contrast with the great mass of details which the book contains. Some idea of the wealth of names of the stars, as well as the assiduity of the author in collecting them and defining them, is shown by the following table, which gives the number of synonyma for twelve stars taken at random from the list of twenty whose magnitudes are greater than the second :—

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* 'Star Names and their Meanings,' by Richard Hinckley Allen. G. E. Stechert: New York (London, Leipzig, Paris), 1899.

α Canis Majoris	71	β Orionis	29
α Argûs	41	α Eridani	15
α Centauri	11	α Canis Minoris	23
α Aurigæ	46	β Centauri	8
α Boötis	42	α Virginis	72
α Lyræ	23	α Scorpii	50

The text is arranged alphabetically under the constellation name, and gives first a history of the constellation, and secondly that of the various star-names contained in the constellation. That these are put alphabetically is a decided advantage over the more usual method of right ascension or polar distance, as has prevailed from the days of Aratus to Elihu Burritt. The use of bold-faced type for the leading names—a device of modern typography—makes them stand in sharp relief from the page to readily catch the eye. The quotations of verse are fresh and apt, showing independent reading no less than the text. Allusions to modern points of interest, such as spectroscopic researches, orbital motions, &c., are frequent and trustworthy—for throughout the entire book it is obvious that the author has taken the pains in most cases to consult the original publications, both ancient and modern. Sometimes secondary publications have been quoted and reference given to them erroneously as authorities; sometimes one astronomer is credited with originating a star-name, when another, anterior to him, often by many years, should be the one historically credited: but these are infrequent errors. The numerous misplacement of breathings and accents on nearly all the Greek words mars the typography for some eyes, but will probably be considered a slight offence to the majority of readers.

The author modestly disclaims that his book is intended for the "professional astronomer, who, as a rule, cares little about the old designations of the objects of his study"; and, indeed, the author is not an astronomer. But neither was Sir George Cornewall Lewis, the London barrister, who has written a classic history of Ancient Astronomy; nor Robert Grant, the invalid student, who wrote an equally authoritative account of the origin and progress of Physical Astronomy. Like these, Mr. Allen has produced a classic work, a compendium of stellar nomenclature, which, in spite of his disclaimer, is well worthy the attention of every professional as well as amateur astronomer.

International Latitude Observatory,
Gaithersburg, Maryland,
1901, Nov. 9.

HERMAN S. DAVIS.

Personal Equation.

At the April Meeting of the Royal Astronomical Society a paper was read by Mr. Bryant about Personal Equation, which caused some discussion about chronographic methods of observing, by the Astronomer Royal and Mr. Hollis, this being supplemented by an interesting note in *The Observatory* for May. Mr. Hollis said that his method of observing, taught him by Mr. Dunkin, is to wait until he sees the star actually bisected by the wire, and then he makes the tap, and he thinks that he is one of a very few who do that. According to Mr. Bryant there are at Greenwich two more computers who do the same.

Now, the Astronomer Royal himself said at the same meeting: "In chronographic observing the observer should regulate his action so as to make the sound of the tap correspond with the passage of the star across the wire."

This method of observing transits by chronograph as if *shooting at a bird* has been used and recommended by high and reputable authorities, and is certainly a natural proclivity in beginners. But it happens that at this Observatory ever since there have been personal equation determinations, *i.e.* since 1878, the facts have generally been to show that the tyro wishes to make his equation zero, and therefore he instinctively adopts this method, but that he instantly gets better results, as to accuracy, the moment he adopts Mr. Hollis's method. I have always taught that "observing is recording what happens, not presuming something to be about to happen," which is the same in idea as Mr. Dunkin's rule quoted by Mr. Hollis.

I see also that in his Catalogue of Fundamental Stars for 1900 (pp. 170–171), Prof. Newcomb recommends the bird-shooting method. He quotes Gould and Gill, the first saying: "The true method to be aimed at, in chronographic observation, clearly is to give the signal at the instant when the star is actually seen to be bisected," and the latter endorsing it; but for himself he says: "When he [Newcomb himself] commenced work as an astronomical observer in 1861, he adopted this very method of waiting until the star seemed to be actually bisected, and then making the tap. The result was a very large accidental error in his observations, which was not diminished until, on the recommendation of Prof. Yarnall, he adopted the method of tapping the chronograph as if shooting at a passing bird."

This, I venture to say, is quite the reverse of my whole and lengthy experience with all the astronomers who have begun their transit observing here, the hydrographic officers who are obliged to train for six months here, and very many other navy and army officers. But I am bound to think the further statements of Prof. Newcomb still more remarkable: "At the same time he adopted a system which he thinks might possibly, with due care on the part of the observer, nearly eliminate the error in question. This consisted in noticing, at each tap of the key, whether the sound was simultaneous with the bisection of the star by the thread. If, at the transit of any star, the tap was found to have been made too late, an effort was made to make the next tap too early, and this process of equalization was continued through the series of threads, so that, in the judgment of the observer, the sound of the taps was, in the general mean, coincident with the bisections." No doubt Prof. Newcomb's extraordinary abilities enabled him to improve his observations in this way, but I may say that for the average observer such a rule would be conducive to utter failure in its aim. He himself adds: "The actual elimination of the error in this way is a proposition to be proved, and not taken for granted." *

Since 1878 there have been, as above said, a number of observers at this Observatory who have begun work with our Personal Equation Machine, and as First Assistant until 1890, and Director after that, my unflinching rule has been always to get them to observe first by eye and ear on the classic system as described by the Astronomer Royal and Mr. Hollis; after they are sufficiently at home with this they begin chronographic observations; at first I always tell them only: "record with the key the moment the fictitious star crosses the wire." Invariably they have tried to have a very small equation; they used the "bird-shooting" method; many *naughts* came from the reading off of the chronograph slips, but somehow their probable error was always large. When they had tried their own method long enough, and had become satisfied that it was not suitable for getting the small probable errors their predecessors had reached, I used to tell them: "Don't be anxious to get small values, but only to see very definitely the instant the star is bisected, and *then* deliberately and immediately (but only then) record it." The result has been always, more or less quickly, to bring their equations to a sensibly larger negative figure than formerly, but its probable error to a much smaller and steadier amount.

Unfortunately I am not able now to give numerical proofs of what I say, for I did not anticipate this, and I never kept a record of the precise time when each observer began the second method. This is now a fact of unfailing experience with me, sometimes even reluctantly accepted, but of which I remember no exception. Probably there is to be found some jump in the series of equation determinations by each observer, but it is not easily to be proved that this corresponds exactly to the change of method; I can only state this as a personal reminiscence. For some of the last instances I very closely remember the facts: the disappointment of the beginners never to be able to improve their probable error; their disbelief in the new rule I advised them to adopt; and their satisfaction in finding its actual efficiency.

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* I may add, by-the-bye, that I have often noticed too in myself and others the effect of the star *jumping* sometimes over the wire without appearing bisected, which Prof. Newcomb felt to be a result of fatigue consequent upon continuous observing work.

The advantages of the Dunkin-Gould-Hollis method are to my thinking very decided. First, it gives (in observing equatorial stars) a greater constancy, as Dr. Gill quoted by Prof. Newcomb says, because there is nothing left to mental-appreciation of different concurring phenomena, but only of one: the bisecting of the star. The taps may be—and are preferably—unheard; the chronograph may be near or far from the hearing of the observer, for its sound is not indifferent if the observer wishes the tap-beats of the key to coincide with the actual bisection he sees. Stars of any declination are observed with nearly the same personal equation in time, which becomes almost impossible with the other method. The field may be darker or brighter, or the stars themselves may be, without probably affecting the observations; in fact it has often happened here that a gust of wind has put out the field-light, but the observer went on observing, following the star-image and recording the instants it became bisected by the dark, otherwise invisible wires; and such observations have never shown any sensible disagreement with the others made in the same transit still with bright field. I am now about to begin trying magnitude equation determinations directly made with screens. I think there is certainly a magnitude effect always, especially for very bright or very faint stars, according to what I guess from experience; but I venture to forecast there will be by this method a smaller range.

As a further explanation I wish to state that some peculiar devices are in use here that may contribute to the result here unvaryingly noticed. The signals are always "breaks," the keys being flat springs (Krille's form) which the observer presses some time before and releases instantly as the actual record; a peculiar arrangement of the chronograph coils and pen allows the signals to be always visible, without any obliteration of the clock-signals however long the key may be pressed down by the observer before he actually gives the break, and notwithstanding the fact that they are all recorded by a single pen. In this manner irregularities in the attractive force of the coils, or electro-motive power of the battery, in the strength of the key-spring, in the distance the contacts are apart, &c., are, I think, if not eliminated, at least greatly diminished.

Royal Observatory, Lisbon, Tapada.
1902, January 20.

CAMPOS RODRIGUES.

[By a somewhat curious coincidence since the above has been in type, a note referring to one of the points mentioned above has appeared in the *Astronomical Journal* (No. 516). Prof. Lewis Boss, Director of the Dudley Observatory, Albany, U.S.A., used to observe transits by the "bird-shooting" method, and from a discussion of observations made with screens in the years 1879, 1880, and 1882, he found that he had a magnitude equation of $0^{\mathrm{s}}\cdot0142$—that is to say, he observed faint stars later than bright ones at the rate of this quantity per magnitude. He then conjectured, as does Signor Rodrigues, that this variation with magnitude would disappear, or would, at least, be reduced, if the other method of observing were adopted. Consequently Prof. Boss changed his habit and observed transits by the method which commends itself to Signor Rodrigues, and when he again made special transit-observations, reducing the light of the star by means of screens before the object-glass, he found that with the new method of observing the equation was $0^{\mathrm{s}}\cdot0141$ per magnitude. The equality of these two results is very remarkable. Prof. Boss goes on to say that as the accuracy of modern meridian observations may be measured by $0^{\mathrm{s}}\cdot02 \sec \delta$ and the magnitude equation by $-0^{\mathrm{s}}\cdot01\,(m-4)$, it is absolutely necessary to determine this last with all precision.—Eds.]

Dr. T. D. Anderson.

Many persons must have been pleased to see the discoverer of Nova Persei at the Annual Meeting of the Astronomical Society, whose name is so familiar, but who had never before put in an appearance at Burlington House. As the President said, it is truly surprising that *both* the sensational "Novæ" of our generation should be associated with the name of one man. Thinking that our readers would like to know more of the gentleman who has received the Jackson-Gwilt medal, a letter was written to him asking for some information about his work, which elicited the following interesting reply with permission to publish.

21 East Claremont Street,
Edinburgh,
Feb. 19, 1902.

My dear Mr. Hollis,

"And must I ravel out my weaved-up"—"follies," I was about to say, like King Richard; but since nothing astronomical could possibly be called foolish, I shall alter Shakespeare and say "my weaved-up past?" Of course I am reluctant; but your request is a sufficient excuse for any apparent lack of modesty on my part.

I need hardly say that before the advent of Nova Aurigæ my astronomizings were fruitless—fruitless, that is to say, so far as the rest of humanity was concerned, but far from being fruitless as regarded myself, for there was for me at least a certain joyful calm when, after a long evening spent in writing sermons or in other work, I threw up the window and taking out my little pocket-telescope surveyed the never palling glory of the midnight sky. But after the appearance of Nova Aurigæ, the thought occurred to me that perhaps after all new stars might not be such rare phenomena as had up to that time been supposed. The correctness of that surmise has been proved by Mrs. Fleming's discovery since then of no fewer than five of these objects on the Harvard College photographs, although it is certainly strange that all these Novæ should have appeared in the Southern Hemisphere. I therefore resolved to commence a search for new stars, a resolve in which I was encouraged by Professor Copeland, the Astronomer Royal for Scotland, who gave me advice as to instruments, charts, catalogues, &c., and told me that my search, if unsuccessful so far as Novæ were concerned, would almost certainly lead to some discovery or other.

Accordingly I at once purchased a large binocular. This I soon reinforced by a second-hand 2¼-inch refractor by the well-known Jesse Ramsden, which shows me stars down to the tenth magnitude; but desiring to see still fainter objects, I bought in 1899 a 3-inch refractor by Mr. William Hume of this city. With this last instrument I can on a clear night see stars as faint as the eleventh magnitude. As regards charts I was somewhat unfortunate, for I had the mortification of learning, when I sent to Bonn for those of the B.D., that many of them were out of print. However, I secured the whole of them for the sky north of +40°, and I filled up the lacunæ among those for the more southerly regions by means of some constructed by myself with the help of the positions given in the B.D. Verzeichniss. These home-made charts, containing as they do upwards of 70,000 stars, cost me a deal of time. It was a work which I should scarcely have undertaken had I known that a new edition of the B.D. charts would appear in a few years. In the matter of catalogues, too, I was somewhat unsuccessful, since all my efforts failed to obtain for me that part of the B.D. Verzeichniss which contains the stars north of +41°. Still, as I had the charts for that part of the sky, the want of the 'Verzeichniss' was little felt by me, and I subsequently supplied any deficiency in that respect by purchasing the various volumes of the great Gesellschaft Catalogue as they came out.

Thus armed I began to hunt for new stars. I worked with might and main, never going to rest as long as the sky remained clear, and often rising in the night to see if the clouds had passed away, and, if they had, hurrying downstairs to begin work either with binocular or with telescope. The chief obstacle that I have had to contend with in such work is that the only windows in this house from which I can thoroughly examine the heavens face the north-west. Not only is my field of labour thereby very greatly circumscribed, my telescope being able to command only that part of the heavens which extends from the equator to +70°, but the discomfort is frequently not inconsiderable, as the northerly and north-westerly winds which so often bring with them transparent, unclouded skies, are in winter and early spring far from being balmy, and can make themselves felt even when the window-shutters are partially closed.

At first my search was mainly for *Novæ* and was prosecuted by means of my binocular, but after several months passed in diligently comparing the heavens with my charts, I found nothing that could be mistaken for a *Nova* except the 8th magnitude star Weisse-Bessel XIX^{h} 358 (otherwise 4767 of the First Glasgow Catalogue), which through some oversight is omitted from the first edition of the B.D. charts. As this star lies in the constellation Aquila between the two branches of the Milky Way, I pounced on it at once as a *Nova*, only to find to my disgust that it was a well-known and long-established denizen of the firmament. (This star has, I may mention, been inserted in its proper place in the new edition of the B.D. charts.) When I came to see that hunting for *Novæ* was not attended by the success which I had anticipated, I began, without entirely abandoning such work, to make a systematic search for variable stars. For this I used my 2¼-inch telescope, comparing what it showed me with the representation of the heavens contained in the B.D. charts. Of course there were very many discrepancies between the two, for it must be remembered that Drs. Argelander, Schönfeld, and Krueger, when making the B.D., never intended that it should be complete except for stars of the ninth magnitude and brighter. Of fainter objects —and, as those who use the B.D. know, these form a large proportion of the stars laid down—there is only a selection. In many instances stars little brighter than the tenth magnitude have been inserted, while others nearly of the ninth magnitude have been omitted. It can easily be imagined how puzzling this was to me, especially as it was to a large extent faint stars that I examined for variability. I was always glad if, after three or four months of searching, during which I might have examined perhaps 20,000 stars and suspected fifty or sixty of variability, I was able at last to come across one whose brightness changed.

I found Nova Persei, I need hardly say, without either binocular or telescope, when I was casting a casual glance round the heavens.

I enclose a list of the variables discovered by me (p. 127). The positions are taken from Dr. Hartwig's Ephemerides for this year, except in the case of the three unnamed ones, *i. e.* the three last found. Two of these three are B. D. stars, and their places are taken from that work. The place of the third one, — Ophiuchi, is the same as that which I have given in *A. N.* 3731. The average magnitudes at maximum are mainly from my own knowledge of the past history of these variables.

I have perhaps been too diffuse in answering your queries, but possibly something that I have written may be of use to you in one way or another.

	Name.	R.A. 1855·0.	North Decl. 1855·0.	Mag. at Max.	Variability announced in
		h m s	° ′		
1.	X Andromedæ	0 8 30	46 12	8½	1900
2.	T Andromedæ	0 14 50	26 11·4	8	1893
3.	V Andromedæ	0 42 13	34 51·8	8½	1896
4.	RR Andromedæ	0 43 30	33 35	9½	1901
5.	U Andromedæ	1 7 14	39 57·0	9½	1895
6.	Y Andromedæ	1 31 8	38 36·3	9½	1900
7.	W Andromedæ............	2 8 25	43 37·8	7	1899
8.	Z Tauri	5 44 6	15 45	9	1900
9.	X Aurigæ	6 0 54	50 14	8½	1900
10.	S Lyncis	6 32 3	58 2·7	9½	1897
11.	X Geminorum	6 37 50	30 25·2	8	1897
12.	V Ursæ Majoris	8 57 54	51 42	9½	1901
13.	T Canum Venat.	12 23 1	32 18·3	8½	1897
14.	RU Herculis...............	16 4 10	25 27·1	7½	1896
15.	— Herculis	16 25 50	7 8·9	9	1901
16.	RV Herculis...............	16 55 2	31 26·4	9½	1897
17.	RT Herculis	17 4 58	27 14·3	9	1896
18.	RS Herculis	17 15 38	23 3·9	8	1895
19.	— Ophiuchi	17 49 39	11 11	9	1901
20.	RY Herculis...............	17 53 28	19 29·7	9	1899
21.	V Draconis	17 55 24	54 52·6	9	1900
22.	W Lyræ	18 9 54	36 37·4	8	1896
23.	V Lyræ	19 3 24	29 25·8	9½	1895
24.	U Draconis	19 9 54	67 2·4	9	1897
25.	TZ Cygni	19 12 12	49 55	9½	1901
26.	RT Aquilæ	19 31 10	11 23	9	1897
27.	RV Aquilæ	19 33 48	9 35·4	9	1900
28.	RU Aquilæ	20 5 56	12 33·8	8½	1898
29.	SX Cygni	20 9 45	30 37·9	9	1899
30.	X Delphini	20 48 13	17 5·6	8½	1895
31.	R Equulei..................	21 6 15	12 12·4	8½	1900
32.	X Pegasi	21 14 8	13 50·3	9	1898
33.	— Pegasi	21 37 56	24 20·6	8½	1901
34.	Y Pegasi	22 4 36	13 38	9½	1900
35.	V Cassiop....................	23 5 27	58 53·8	8	1893
36.	W Pegasi	23 12 34	25 29·1	8	1895
37.	Z Cassiop................ .	23 37 30	55 46·6	9½	1898
38.	RR Cassiop.................	23 48 24	52 55	9½	1900

Yours very truly,
Thomas D. Anderson.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Reduction of Hornsby's Observations.

Gentlemen,—

In the February number of the *Observatory* Professor Turner explains the reasons which have led him, for the last three years, to oppose my application to the Government Grant Committee of the Royal Society (of which Committee he is a member) for a grant of money to pay the salaries of computers to reduce the old Radcliffe observations made by Hornsby between 1774 and 1803.

I may perhaps be permitted to express the satisfaction I feel in learning that he now wishes to range himself alongside me in hoping that they may be published. I should like to assure him that any movement in that direction will be cordially welcomed by me. But, since his opposition to my scheme is, I believe, the only obstacle that stands in the way of that wish being accomplished, I cannot but regret that his interest and sympathy in the project should still be of only a "sentimental" and "abstract" character.

Even if it were a matter likely to interest your readers it would be decidedly indecorous for me, being at the present moment an applicant before the Committee, to discuss in public, with one who is a member of that Committee, the technical points of my scheme, with regard to which he and I hold such different opinions. In this connection it is only fair to Professor Turner to state that, when writing his letter, he probably did not know that I intended to apply again this year, as my application was not sent in until January 31.

But I cannot so easily exonerate him for the light in which he represents my attitude in the matter; and with regard to this point I desire to offer a few corrections to Professor Turner's statements.

In the second paragraph of his letter he writes, "They [*i. e.* his reasons] have been fully explained to Dr. Rambaut himself, without eliciting any reply from him," and, "so far as I know, Dr. Rambaut has hitherto made only one specific application, to which there are, at least, two grave objections; these have been pointed out to him and he has offered no reply, but merely repeated his application in the same terms." In the last paragraph he writes: "I am sorry to have to discuss such a matter in public; but private discussion has failed."

Now, I should like to point out that the "private discussion" consisted in a single visit which Professor Turner paid to the Radcliffe Observatory towards the end of 1900 (at that time my application had already been twice refused), in which he explained to me his two chief objections to my scheme. These were (1) that he doubted whether these observations (which up till that time he had not even looked at) were worth reducing at all, and (2) that he considered my estimate excessive, and that he could not recommend my application to the Committee unless I consented to adopt his method of employing juvenile computers at £36 a year or even less.

So far from recognizing the importance of these objections, I told Professor Turner at the time that they did not appeal to me at all; but, as he offered to waive the first if I could see my way to meet him with regard to the second, and as I was most anxious to do anything in reason to facilitate matters, I undertook to consider his suggestion. This accordingly I did. I also sought advice from many astronomers of experience, with the result that I was more than ever confirmed in my first view of the case. Subsequently, I wrote a letter to the Committee, which came before it along with my application, stating clearly my reasons for thinking that there would be no economy in employing boy-computers as suggested by Professor Turner, and supporting my arguments by letters from two of the most distinguished British astronomers, one of whom in the course of his letter wrote "I do not think it would be right to employ boys at £3 a month on the reduction of Hornsby's observations." Can Professor Turner not have read this letter when he wrote that his representations failed to elicit any reply?

Later, happening to meet Professor Turner, I briefly informed him that I had been unable to alter my estimate and that it must go forward unchanged.

In the same letter to the Committee I stated clearly that in case my application were successful I intended to put one of my permanent assistants on the work, who, under my own supervision, would direct, superintend, and check every step of it. This would have been a very important contribution on the part of the Radcliffe Observatory towards the reduction of these observations. And yet Professor Turner writes, "Is it impossible to do these reductions, or at any rate a considerable part of them, as the ordinary work of the Radcliffe Observatory?"

Professor Turner suggests that I should stop the transit-circle observations for a few years in order to bring up these "important old arrears." If, however, he had re-read the report of the Radcliffe Observatory for last year (which he quotes in another paragraph) he would have seen it stated in the opening sentence that I had already been obliged to follow Sir David Gill's excellent example, and to discontinue transit-circle observations in order to overtake the arrears of reduction left by my immediate predecessor. Does Professor Turner maintain that such work ought to be indefinitely postponed, or that the fine 24-inch photographic refractor, which is in process of erection here, should be allowed to stand idle for a number of years because he differs from me (and from many other astronomers) as to the economy of employing computers who have at least had some experience in computing?

I do not propose to offer here any discussion of the main question at issue between Professor Turner and myself, viz. the probable total cost of the undertaking. A statement of my views on this subject will come before the Committee of the Royal Society in due course. With regard to the Greenwich system of employing boy-computers, which Professor Turner recommends, I shall merely quote a sentence bearing on the subject from the Astronomer Royal's report to the Board of Visitors last year. The Astronomer Royal writes: "Within the last five months one-third of the whole staff of computers have left the Observatory for other posts and have had to be replaced by boys new to their work. Such an extensive change in the temporary staff has, to a certain extent, disorganized the work and has thrown a great strain on the Assistants, who are charged with carrying it on under such difficult conditions. Considering the training and experience required in the varied work which, at Greenwich, has to be done by computers, a greater degree of permanence in the staff appears to be necessary for the continued efficiency of the Observatory."

My principal object in writing now is to clear up some points which seem to have escaped Professor Turner's attention. But I should like to state that the number of observations which I propose to reduce amounts to close on 100,000, and that the sum I ask for is, *not* one half the total grant for all the sciences put together, as might be hastily concluded from Professor Turner's letter, but one-twentieth part of it (viz. £200 a year) for ten years. I am content to leave astronomers to judge whether this is an extravagant estimate or not.

I should also like to take this opportunity of pointing out that Professor Turner does not represent the general attitude of British astronomers towards these observations as the wording of his last paragraph might seem to imply, and to explain that in writing the sentence (quoted by Professor Turner), "Are astronomers content to leave all this material unused, &c.," my obvious intention was to appeal directly to the opinion of the majority of astronomers in this and foreign countries, whom I have good reasons for believing to be favourably disposed towards, and much interested in, the reduction of these important old observations.

Radcliffe Observatory, Oxford, Yours faithfully,
1902, February 17. Arthur A. Rambaut.

The alleged Change of Colour in Sirius.

Gentlemen,—

Prof. Kreutz has kindly written to me with reference to my letter in your January number on the above subject, calling my attention to Prof. Schiaparelli's two papers on "Rubra Canicula" which appeared in 1896 and 1897. Prof. Newcomb, in the recent work to which I referred in that letter, also speaks of these papers and expresses a decided preference for the view taken by their author over that of Prof. See as the result of an investigation published shortly before in *Astronomy and Astro-Physics*, to which also I referred. I gather from the way in which Prof. Kreutz characterizes Schiaparelli's discussion, "the most important written on this subject," that he also gives the preference to the conclusion of the latter.

The idea that a change had occurred in the colour of the star, first made by Barker, was affirmed with emphasis by Humboldt in the third volume of the 'Cosmos,' Stuttgart Edition of 1870, vol. iii. p. 111, where he says:—"Sirius gewährt demnach das einzige Beispiel einer historisch erwiesenen Veränderung der Farbe, denn er hat gegenwärtig ein vollkommen weisses Licht."

Sir John Herschel accepted this change as a fact, but thought it so remarkable that he suggested (*Ast. Nach.* No. 372, vol. xvi. col. 188, quoted by Schiaparelli at p. 3 of his first paper) that it was caused by the interposition of a cosmical cloud capable of absorbing a portion of the rays of various colours which produced the light of the star*.

In an earlier communication of mine (*The Observatory*, vol. x. p. 104, to which Miss Clarke does me the honour to refer in her 'System of the Stars,' p. 146), I called attention to the mistake (pointed out by Schjellerup) which had probably been made in supposing that Ptolemy had classed Sirius amongst the red or reddish (ὑπόκιρροι) stars. But later (particularly Roman) writers are generally regarded as having done so. I am extremely obliged to Prof. Oom, of Lisbon, for calling my attention to the fact that a main point in Prof. Schiaparelli's paper is the strong probability that by Canicula these writers do not mean Sirius, but Procyon.

After a full discussion of the passage in the 'Almagest,' Prof. Schiaparelli goes on to speak of the Roman writers. Cicero, Germanicus, and Avienus published translations of Aratus †, and in these the expression "rutilus" is applied to the light of Sirius. But this word does not necessarily mean red or even any tint of colour; Cicero applies it in the same translation to the body of the constellation Virgo, evidently meaning simply "bright," as Schiaparelli remarks "*senza* designazione di colore speciale." The decided attribution of a red colour to the star called by them Canicula is to be found in Horace and Seneca. The former speaks of "rubra Canicula" in Sat. ii. 5; the latter, in his 'Quæstiones Naturales,' i. 1, says that it is not wonderful there should be varieties of colour on the Earth since such exist also in the heavens, "sed acrior sit caniculæ rubor, Martis remissior," which has been generally taken to mean that Sirius is more red than Mars. But, as I remarked before, Prof. Schiaparelli gives reasons for believing that Canicula means Procyon, not Sirius. It will be sufficient here to quote the passage to which he refers from Hyginus: ". . . . canem autem sua appellatione et specie caniculam dixerunt; quæ a Græcis quod ante majorem Canem oritur Procyon appellatur."

In his second paper, Prof. Schiaparelli states that when he wrote his first he had not seen those of Prof. See. He therefore now goes over the same passages as the latter had done, and contends that in the other ancient authorities where Sirius is alluded to, the epithets applied to it refer to its brightness, not its colour; thus in the 'Iliad,' where it is compared to the arms of warriors (Diomed, Hector, and Achilles), and likened to the appearance of fire, what is meant is not redness, but power of light, inextinguishableness, as the poet himself expresses it, ἀκάματον πῦρ (Il. v. 4).

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* "It seems much more likely that a red colour should be the effect of a medium interfered [so in the original, but Prof. Schiaparelli quotes the word as "interposed," and it seems likely that "interfered" is a misprint] than that in the short space of 2000 years so vast a body should have actually undergone such a material change in its physical constitution."

† That writer's own epithet is ποικίλος.

Prof. Schiaparelli's arguments appear to be very convincing; but is there not another difficulty introduced with regard to the passages from the Roman writers, especially Seneca? Must we take him to mean that *Procyon* was red, and, according to Seneca, of a hue "acrior" than that of Mars? Perhaps on this point I may again refer to my letter to you of Feb. 1887 (vol. x. p. 104), at the end of which the suggestion is mentioned that the true reading is not "rubor," but "fulgor." Not even Sirius is so bright as Mars when near opposition, but the comparison may have been made when the planet was near quadrature.

Yours faithfully,

Blackheath, 1902, Feb. 3. W. T. LYNN.

The Eclipse of Thales.

GENTLEMEN,—

The date B.C. 585, adopted by Mr. W. T. Lynn in his late letter in the *Observatory*, is that usually to be found in text-books since the late Sir G. B. Airy's investigation of the subject; and I find, for example, this date treated as definitely fixed in the last edition of Chambers's 'Descriptive Astronomy' and of Herschel's 'Outlines.' But I now learn that a new investigation by Prof. Stockwell results in fixing the date as either B.C. 603 or B.C. 610, and that he decides, on non-astronomical grounds, in favour of the latter date, which was generally accepted before the investigations of Airy. Permit me to remark, in the first place, that Herodotus does not state that Thales was living at the date of the eclipse. He left disciples who would treasure his predictions, and the verification of one of them after his death—like the reappearance of Halley's Comet—would probably be regarded as even more impressive than if it happened in his lifetime. This allows a wider scope for the possible date. Next I think the strong likeness between the record of Herodotus and that of Xenophon should not be overlooked. According to both narratives, the Medes were one of the parties engaged in hostilities, and the hostilities were terminated by the eclipse. The difference is that Herodotus makes the enemy the Lydians, while Xenophon says the Persians, and one speaks of a battle and the other of a siege. But is it probable that a war in which the Medes were engaged should be twice terminated by a total eclipse of the Sun within half a century? Airy adopts B.C. 557 as the date of Xenophon's eclipse, but there appears to be a doubt as to whether there was a total eclipse at that date. I do not know what others have been assigned. I may remark that the fulfilment of Thales's prediction would hardly have been recorded if the eclipse had not been total (or nearly so) at Miletus. Schwegler, in his 'History of Philosophy,' assigns to Thales's life the dates B.C. 640–550.

It seems to me to be almost wasted labour to endeavour to fix the dates of these early eclipses without better historical data. Further excavations may lead to the discovery of inscriptions throwing light on the subject. Without them we seem powerless.

Truly yours,

Dublin, 1901, Dec. 14. W. H. S. MONCK.

OBSERVATORIES.

HARVARD.—Prof. E. C. Pickering's report for the year ending 1901, September 30, opens with a reference to the three events of the astronomical year:—In the observation of Eros at Opposition the observatory did not join, as there were already so many engaged in this campaign that additional observers did not seem necessary; the authorities at Harvard also did not deem it advisable to risk a large sum in sending an expedition to observe the Eclipse and to take the chance of cloudy skies. As to Nova Persei, the Director makes the incontrovertible remark that it is much more difficult to discover a faint star of this class than a bright one, and that of the eight Novæ observed during the last fifteen years, six, in fact all the faint ones, were discovered at Harvard.

The work with the various instruments has gone steadily on much as in former years, and it may be sufficient to simply record here the number of observations made with each for comparison with former years.

With the polarizing photometer on the east equatorial, 1224 comparisons of the light of Nova Persei were made, the light of the planet Eros was determined on 50 nights, and besides these about 10,000 comparisons of the magnitude of variable stars and of small planets were made. With the 12-inch meridian photometer, 54,448 settings were made on 126 nights by Prof. Pickering, which is much less than usual, owing, for one thing, to cloudy weather, and also to the fact that the stars observed were fainter than usual, which made their identification and measurement more laborious. The other meridian photometer has been used by Prof. Solon Bailey to make a catalogue of 376 standard stars of about the fifth magnitude, one in each region 10° square. The number of settings made during the year was 33,316.

Not much work was done with the meridian circle. The energy of this branch being devoted to the completion of the Zone Catologue $-9° 50'$ to $-14° 10'$.

The taking of photographs of spectra of stars with the 11-inch Draper telescope continues, and 673 plates have been obtained during the year. But the number of objects having peculiar spectra detected by Mrs. Fleming this year is less than usual, as her time has been devoted to the preparation of a volume of the 'Annals,' which will contain photographic magnitudes of 195 variables discovered at Harvard, on photographs taken from 1885 to 1901 inclusive. The following two paragraphs relating to this section seem worth reprinting; the first may serve as instruction to other observers, and the second shows what a complete watch over the sky is kept at Harvard:—

> Several novelties have been introduced. For example, the spectrum of Nova Persei, when within 10° of the horizon, has been photographed with an objective prism, the latter being turned by a computed amount so as to correct for the atmospheric refraction in declination. Photographs of stellar spectra have been shaded automatically, so that the ends are as intense as the centre, by interposing contact prints and porcelain screens.
>
> In the systematic charting of the sky the 8-inch Draper telescope covers all parts north of declination $-12°\cdot5$ from two to four times a year; the Cooke lens covers all parts available at this latitude two or more times a month; and the Transit Photometer furnishes on every clear night a record of all stars crossing the meridian and visible to the naked eye. On the best plates of these three series of photographs the faintest stars visible are of about the thirteenth, twelfth, and sixth magnitudes, respectively. In extension of this scheme, work has been inaugurated with a small Ross-Zeiss lens which covers a field about 60° square. An exposure of one hour shows stars as faint as the eighth magnitude; and all visible in a single night can be photographed by exposing seven plates. Similar series of photographs of the southern stars are taken at Arequipa.

The Bruce photographic telescope has been used to photograph planets and comets and other special objects. This instrument is under the direction of Dr. De Leslie Stewart, whose name in this capacity seems new to us. From the examination of the plates he has found 298 new nebulæ during the year. There has been made at Harvard a long series, extending over 22 years, of photometric observations of the eclipses of Jupiter's satellites. Prof. Pickering has enlisted the co-operation of Prof. Sampson, of Durham, in the reduction of these, and, as the Report says, "the value of the final work will in this way be greatly increased." Dr. Doberck, of Hongkong, has also consented to make a second reduction of Sir J. Herschel's observations of the light of southern stars, so as to reduce them to the scale of the meridian photometer.

WASHINGTON U.S. NAVAL.—The Report of this Observatory for the year ending 1901, June 30, is signed, as those for previous years, by Capt. C. H. Davis, U.S. Navy, Superintendent of the Observatory, and is followed by the Reports of the work done with the various instruments made to him by the astronomers in charge of each. The Superintendent notes in his Report that Prof. S. J. Brown was detached on 1901, March 25, from the Observatory, and from his position as Director of the Nautical Almanac; and that Prof. W. S. Harshman, who has been for some time in the office of the Almanac, was appointed Director in his stead. In connection with these facts should be read our abstract of the report of this observatory in the number for last March and a note on page 212 of our number for last May, which reports a charge of certain insubordination made against the Director.

Turning to the Reports of the astronomers in charge of the instruments, Prof. See has a long list of work done with the 26-inch equatoreal. His measures of the planets and satellites have been published in other places, and have been briefly referred to in these pages. Some micrometer measures of the position of Eros and of some double stars have also been made. Prof. See has had his work increased, first by the lamented death of a promising astronomer, Mr. Lawton, who helped him, and by the absence of another assistant who went with the eclipse expedition.

It may be remembered that this observatory has been supplied recently with a new 6-inch transit-circle, and with an altazimuth with 5-inch object-glass. The first of these seems to be still in the experimental stage which all new instruments have to pass through. It was made by Messrs. Warner & Swasey after the pattern of the latest instruments made by Messrs. Repsold, the pivots resting on a skeleton framework of metal by which they are raised sixteen inches above the piers. The collimation and level constants change very slowly, but the azimuth at first showed very large changes with temperature, which various alterations have not entirely eliminated. The old 9-inch transit-circle has been used during the year for various purposes and 6310 observations have been made with it, but there is suspicion of various faults in this instrument, which will be dismounted for repair as soon as the new meridian instrument is in working order. A clock-vault is being constructed, which it is intended shall be kept as nearly as possible at a constant temperature throughout the year.

The new altazimuth together with the prime vertical transit are in charge of Assistant-Astronomer G. A. Hill, who replies in his Report to various criticisms made in some periodicals adverse to these instruments. It appears from his record of work, which has for its end the determination of the Variation of Latitude and the Constant of Aberration, that these criticisms are unfounded. Mr. Skinner's report deals with the results of the eclipse expedition to Sumatra, whilst that of Prof. Harshman, on the computations of the Nautical Almanac, states that new tables for Jupiter's satellites are being prepared, which is welcome news.

PUBLICATIONS.

Geschichte des Fixsternhimmels.—The annual report presented by Dr. Ristenpart to the Berlin Academy of Sciences on January 23, gives the progress made during the year 1901 in this undertaking. Those who cannot conveniently refer to the

Proceedings of the Berlin Academy will find an account of the intention and scope of this work in a note by Dr. Auwers in the Council Report of the R. A. S. for 1901. Briefly stated, it is proposed to collect all meridian observations of stars made from 1750 to 1900, and to form from them a general catalogue for the epoch 1875·0. It is estimated that a million catalogued places of 250,000 stars from 300 catalogues will be used in forming this work. At the beginning of 1901 the catalogues from 1750 to 1842 had been dealt with, yielding 240,000 positions. During 1901 the epoch 1842 to 1855 was similarly treated, and gave 112,000 positions. In addition to catalogues already published, Dr. Ristenpart is making efforts to get unpublished observations reduced, so that his catalogue will be complete. Dr. Weiss has sent him a manuscript catalogue of 4165 stars observed at Geneva about 1850. The formation of a catalogue of 6132 stars from the Cambridge observations made between 1847 and 1869 is in progress at that University, while Dr. Ristenpart himself is forming a catalogue of 1516 stars from the observations made between 1836–1846.

Dr. Ristenpart also began the formation of a catalogue for 1850 from the Washington zone-observations begun in 1846; but this work, which is more conveniently done by those having access to the original manuscripts, was afterwards undertaken by the Naval Observatory.

This important undertaking of the Berlin Academy has been begun at a very opportune moment. The International Photographic Catalogue of the Heavens will very shortly furnish the positions of thousands of stars for the epoch 1900 with very great accuracy. The first thing to be done with these results will be the comparison with earlier observations. Dr. Ristenpart's catalogue will simplify this work enormously, and at the same time lead to its being done more thoroughly and completely.

F. W. D.

NOTES.

Comet Notes.—Prof. Brédichin has published in the *Bulletin de l'Académie des Sciences*, St. Petersburg (1901 Dec.), an important memoir on the great comet of last year. Dr. Gill sent him 13 glass positives of photographs taken at the Cape, and these have been carefully measured by M. Kostinsky to obtain the position and rate of motion of various points in the three tails These latter form examples of Types I., II., III. of his now well-known classification, the third type being represented by the large faint, lateral tail to the south of the two main tails.

The measures of various points are given at length, and Prof

Brédichin analyses them in accordance with his theory of the formation of comets' tails by expulsion from the nucleus combined with the repulsive force.

He finds evidence of a violent eruption, driving matter outwards from the nucleus, which took place on April 23. This eruption appears to have lasted several hours, from the great width of the bands resulting from it.

After this the nucleus appeared destitute of *chevelure* and envelopes, which is attributed to the weakening of the repulsive energy after the great outburst.

Prof. Brédichin considers it probable that the southern edge of the faint tail may really have been slightly curved, as we should expect from theory; but it was too faint for this point to be noted.

He thinks that the explosion of April 23 probably caused a rotation of the nucleus in the same direction as that of the orbital motion.

A much feebler eruption seems to have taken place on June 2, soon after which the comet was lost to view.

The whole memoir deserves the careful study of all who are acquainted with Prof. Brédichin's methods of analysing cometary phenomena.

Ast. Nach. 3763 contains a determination of the orbit of Comet 1845 II., by Dr. A. Schneller.

The observations employed are about 130 in number, and extend from Feb. 25 to April 23, 1845. Eight normal places are formed from them, from which the following elliptical elements are deduced :—

			Prob. error.
T	1845 Apr. 21·0549 Paris M.T.		±0·0117
π	192° 34′ 45″	1845·0.	±65″
☊	347 7 5		±33
i	56 22 31		±30
log q....	0·09846		±0·00012
log e....	9·99977		±0·00070
Period ..	115,000 years.		

It goes without saying that the elliptical character of the orbit is quite uncertain; a parabolic orbit will satisfy the observations almost equally well.

Ast. Nach. 3770 contains definitive elements of Comet 1898 V. (Giacobini), by Adolf Hnatek; the comet was discovered 1898 June 18, and was under observation for two months. In spite of its faintness the observations are very accordant and the residuals small. It is pointed out that the investigation required special precautions in consequence of the comet's latitude having been so small throughout the period of observation.

The following are the definitive parabolic elements :—

T	1898 July 25·5338 Berlin M.T.	
ω	313° 36′ 34″	1898·0.
☊	212 12 2	
i	155 0 43	
$\log q$	0·176473	

He examines whether an alteration in the eccentricity will produce smaller residuals. He finds an ellipse with a period of 42,043 years, but regards the parabolic orbit as equally probable.

A. C. D. C.

MINOR PLANET NOTES.—A planet, HO, of the 12th magnitude, was found by Palisa at Vienna on Feb. 4; it is probably not new, but identical with No. 458.

Another planet, HP, of the 11th magnitude, was discovered by Carnera at Heidelberg on Feb. 12.

1901 appears to have been the richest year on record for the discovery of minor planets, no fewer than 38 having been added. The discovery of HM has not yet been announced in these columns; it was found by Kopff at Heidelberg on Dec. 5.

GV, announced last year, is not new, but identical with 202 Chryseis.

A. C. D. C.

ZENITH-TELESCOPES.—Designs for apparatus for measuring the distances from the zenith of stars which are not far from that point have lately appeared over two well-known names. M. Cornu gave details of a scheme in the *Bulletin Astronomique* for October 1901, and M. Lippmann presented a plan to the Academy of Sciences on January 27 last (*Comptes Rendus*, tome cxxxiv. no. 4). Any abstract of these that could be made in such limited space as is here available would be insufficient, but the general features which are in some way common to both may be indicated. Each apparatus comprises a mirror inclined at 45° to the horizon, which reflects from its upper surface the stars around the zenith into a horizontal telescope. In the field of this horizontal telescope the nadir is marked: in M. Cornu's apparatus this is effected by having a second mirror at right angles to the first, with half their widths overlapping, so that the second does not prevent the reflection of the stars from the first, and a mercury-trough below, and when the movable horizontal wire is made to coincide with its image after reflection from the two mirrors and the mercury, its place indicates the zenith. Coincidence of the wire with its image after reflection from the two mirrors ensures that these are properly adjusted. In M. Lippmann's method the fiducial point is supplied by a slit in the focal plane of a collimator placed on the side of the mirror opposite to the telescope and a trough of mercury below the mirror ; but the reader must be referred to the original for details of this plan.

HIPPARCHUS AND PRECESSION.—It is generally believed that Hipparchus was the first to recognize the gradual increase of longitude of all the stars which is designated by the expression "Precession of the Equinoxes," but there is not a consensus of opinion as to whether he believed the whole sphere of stars moved in the direction of increasing longitude or whether the tropical and equinoctial points retrograded. The opinions of astronomical writers seem to be nearly equally balanced on this point. A short paper was recently read before the Royal Irish Academy * by the Rev. M. H. Close, in which he finds the evidence to be in favour of the first hypothesis, namely, that Hipparchus thought the stars themselves move. Mr. Close's principal argument is based on Ptolemy's treatment of Hipparchus's tractate entitled "Concerning the shifting of the Tropical and Equinoctial Points." He argues that this title does not imply anything definite about Hipparchus's opinion, any more than when we speak of the Sun *rising* it implies a belief that the Sun actually moves; but Ptolemy knew what Hipparchus thought, and Ptolemy himself writes that the sphere of the fixed stars has its own proper motion, and at the same time makes no comment on Hipparchus's views on this subject, which he would have done if they were opposed to his own. Mr. Close also mentions a fact which is almost conclusive. When Hipparchus had examined certain zodiacal stars, we are told by Ptolemy in the 'Almagest,' he thought that the extra-zodiacal stars did not participate in the progression. This mistake he afterwards corrected and found that all the stars retained their relative positions, but did not make any statement showing that this was due to the movement of the equinoctial points.

ON MEASURING SPECTROGRAMS.—An instance of personal error in measurement which Mr. H. M. Reese calls attention to in Lick Observatory Bulletin No. 15 is worth recording. In measuring lines in star-spectra (bright lines on a dark field) with lines in a comparison spectrum (dark lines on a bright field), it was found that a different result for velocity in the line of sight was obtained according to whether the violet end of the spectra were set on the right or left hand of the measurer. After several other possibilities had been investigated, the cause was found to be due to a psychological tendency of the measurer to set the measuring-wire further to the right on a dark line in a bright field than on a bright line in a dark field.

THE SOLAR ROTATION.—Father Cortie has extracted material from the solar observations made at Stonyhurst, to deduce a value of the rate of rotation of the Sun's photosphere from the drift in longitude of faculæ. This has been done before by Stratonoff and possibly others; but Father Cortie's plan differs from these because he takes the cases of groups of faculæ which have endured for a long time, sometimes for four rotations. The result deduced from thirteen such groups is somewhat in accord with Carrington's figures, which show variation of rate with latitude.

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* 1901, May 13.

TYCHO BRAHE'S OBSERVATORY.—It is three hundred years since the death of Tycho Brahe, and the occasion has been celebrated by excavating his observatory in the island of Hveen. An account of this work has lately been published by Prof. Charlier, of Lund, which Dr. Dreyer summarized in an article in *Nature* for December 5. With reference to the fact that Tycho sought shelter from the wind by placing his large telescope a couple of feet below the ground, Dr. Dreyer says:—"But the example set by Tycho was not followed: for more than a hundred years the object seemed generally to be to get as near to the stars as possible by placing observatories on the tops of towers and high buildings. The nineteenth century reverted to Tycho Brahe's ideas by building observatories at some distance from cities, and with the instruments at very moderate heights above the ground. Another idea of Tycho's, which was not adopted for several centuries, was to have a large staff of assistants, among whom the work of the observatory was divided." The article ends with a paragraph showing that Tycho was probably the first man to conceive the idea of a National Observatory.

THE COLD WEATHER IN FEBRUARY.—For 21 days the weather in February was excessively cold, the mean temperature from Feb. 1 to 21 being 32°·4, or 7° below the average. The lowest indicated temperature near London was 14°·3 on the 16th, being the lowest temperature recorded in February since the severe winter of 1895, when the lowest temperature on record for the preceding 61 years, viz., 6°·9, occurred on the 8th. Other temperatures in February lower than that observed this year have been:—7°·7 on 1845 February 12; 11°·1 on 1855 February 19; 11°·2 on 1847 February 12; 12°·4 on 1841 February 4; and 13°·8 on 1855 February 14. The mean for the eight days February 10 to 17 (this year) was below the average value by 9°·7; but in 1895 the mean for the whole month was 10°·4 below the average, and the mean for the 10 days February 5–14 in 1895 was as much as 16°·6 below the average. The rainfall in the 21 cold days was very small, amounting to 0·219 inch only; but after the 22nd the weather was much wetter and warmer, becoming finer again towards the end of the month.

PROF. P. TACCHINI has resigned the office of Director of the Observatory of the Roman College at Rome, which he has held for some years. He has been succeeded by his chief assistant, Prof. E. Millosevich.

THE next Meeting of the Royal Astronomical Society will be on Friday, March 14; that of the British Astronomical Association on Wednesday, March 26.

FROM AN OXFORD NOTE-BOOK.

IT is long since the R. A. S. had the pleasure of seeing at its Annual Meeting two medallists in person. In the years before 1833 two or more medals were occasionally given in the same year, but it was subsequently decided to award only one annually. In the year 1848, under the stress of quite extraordinary circumstances, twelve testimonials (declared at the time to be the equivalent of the gold medal) were awarded; and joint medals (for practically the same research) were awarded in 1867 (Huggins and Miller) and 1886 (Pickering and Pritchard). Otherwise the practice of a single medal was not departed from till 1897. In that year, besides the award of the gold medal to Prof. Barnard, there was awarded for the first time the *Hannah Jackson* (née *Gwilt*) *Gift* to Prof. Lewis Swift. This gift is the outcome of a bequest made to the Society in 1861, which did not, however, become available till after the decease of the donor in 1893 (see *Mon. Not. R. A. S.* lvii. p. 35), and is after all not a "medal" but a "gift." Prof. Swift did not make any attempt to come to England in 1897, and we all remember Prof. Barnard's unfortunate arrival just too late for the meeting, owing to bad weather on the voyage. Only one of the joint medallists was present in 1886. So that we must go back at least 35 years for an occasion at all resembling that of Feb. 14 last, when both the constructor of the Cape Photographic Durchmusterung and the discoverer of *two* Novæ were present in person to be honoured by the Society.

PROF. KAPTEYN gratified us all by making a stay of a few days in England, and bringing Mrs. Kapteyn with him; and he was good enough to say that he found so much to see that he must leave a good deal for another visit. Dr. Anderson (the ideas of a discoverer of two Novæ are of such public interest that I venture to state the circumstance) returned to his "star-gazing" in Edinburgh very promptly. The big telescopes at Greenwich and elsewhere, he said, did not appeal to him in any way; his instrument is a field-glass, with which he can see a number of stars at once instead of one only as in a big telescope. "I'm not an astronomer," he said; "I am an astro*phil*; wherever the stars are shining I *must* be looking." And he told me, in reply to a definite question, that he actually *looks* for new stars; he knows the heavens so well, especially near the Milky Way, that he does not think one could appear brighter than the sixth or perhaps the seventh magnitude without his detecting it. His discoveries were the outcome of a deliberate and independent research, not connected with his work on variables, for which he uses Argelander's charts and a small telescope. How numerous are the astrophils? I am glad that it has fallen to my lot to meet at any rate one.

WHY, I wonder, are so many French publications not dated? An important memoir by M. Callandreau has just been received in a green-paper cover—"Aperçu des Méthodes pour la Détermination des Orbites des Comètes et des Planètes"; but there is nothing to show when it was written either at the beginning or end, nor to show to what series of publications it belongs (if we may judge from the absence of a titlepage that it belongs to a series). If a careful librarian puts the date of receiving it on the copy, this will be perhaps sufficient for practical purposes; but if it passes on to the shelves undated, it may give some poor researcher in times to come a lot of trouble to assign it to its proper place in the history of the subject. I have noticed this absence of date in other French publications, and respectfully suggest to MM. Gauthier-Villars that they should set a good example of reform.

THE report of the Superintendent of the U.S. Naval Observatory shows that some useful work is being done by some of the assistants, but the part of it written by the Superintendent himself is a rather piteous document. The poor man is apparently harassed by malignant critics, official and otherwise. He records with an obvious sigh of relief that the Board of Visitors have already made the full number of visits (3) allowed by law to any one year, but he has other trials.

"Critics who are in no way responsible for results, and who probably would not carry out their own suggestions if they were, have had a standing grievance against the observatory because it has not put its clocks underground.

"I invite the attention of the Bureau to Mr. Hill's report, as evidence of the spirit in which the observatory has been (and still is) criticised by outside astronomers. Malice has dictated these criticisms, to my knowledge, for thirty years. In this instance ignorance becomes a valuable accessory.

"Scientific work must stand or fail on its merits. But it would be an encouragement, little to be expected, if the scientific world of this country could appreciate or acknowledge the efforts to bring up to date work long in arrears."

The last sentence, where the writer turns from scolding to entreaty, reminds us of the plaintive notice once hoisted over an indifferent pianist in the Wild West: "Do not shoot the pianist; he is doing his best." The question after all is not whether the pianist is doing his best, but whether the United States should put in charge of their national piano a naval officer who knows nothing of music.

The appendix to *N. A.* 1902 gives, as we are informed on the second page, "the following corrections to the apparent places of stars as given in the *Nautical Almanac*, 1902, to $0^{s}\cdot01$ in Right Ascension, and $0''\cdot01$ in Declination." The result cannot be considered satisfactory. To a casual glance it is simply a wasteful piece of printing, for the words "insensible in R.A." are monotonously inserted in a separate column for each star, when a single statement would have sufficed, and saved half the printing, say eight pages. But we demur to the actual statement. It implies that numbers printed for the Paris Conference constants are equally correct (to $0^{s}\cdot01$) for the Struve-Peters constants. Now, although the difference between the two sets may never amount to $0^{s}\cdot01$, the figures suitable for the two sets would certainly often differ by $0^{s}\cdot01$, according to the proper raising of the figures omitted. If astronomers use the *N. A.* 1902 for star-corrections, they will certainly get results which make their observations in 1902 not comparable with 1900 and previous years.

It is to be hoped that Mr. Thomas Thorp will perfect his new "polyslit spectroscope," referred to on p. 158 of the *B. A. A. Journal*, vol. xii. If I understand rightly the very brief description there given, the instrument produces a monochromatic image of the Sun without some of the defects of the spectroheliograph, or of other devices recently suggested for getting such an image. A number of slits are "mounted on the periphery or flange of a rotating disk in such a manner that the slits on opposite sides of the disk are conjugate to each other when in the focal plane of the collimating lens and eyepiece respectively." This would seem to secure a very perfect automatic correspondence if the slits can be cut accurately enough. At present this accuracy has not been attained; but we read with pleasure the final sentence, "Mr. Thorp intends to construct such an instrument when sufficient leisure is afforded him."

A Hong-Kong correspondent kindly sends some amusing cuttings illustrating the behaviour of the Chinese with regard to eclipses. Two translations of an official notice are given below, of rather different merit; and a newspaper paragraph received later informs us that on November 11 the officials duly assembled, knelt down facing the setting Sun, and remained motionless for some fifteen minutes, when the rescue of the Sun was announced. During the ceremony there was beating of drums and gongs to frighten away the dragon. These are the translations:—

(1) *A Notification in the matter of Saving and Guarding the Sun and the Moon.*

On the 10th day of the 9th Moon (1901 October 21), Yune, Intendent of Circuit, invested with power of control over military forces of Suchow, T'sung Kong, and Tai Tsong, issued a notification, suspended in front of his Yamen, to the effect that on the 15th day of the 9th Moon (1901 October 26), being the Mo Yan day (of the Diurnal Sexagenary Cycle), there will be an eclipse of the Moon (literally, an eating of the Moon), and that on the 1st day of the 10th Moon (1901 November 11), being the Kwai Tsz day, there will be an eclipse of the Sun (lit., eating).

On the arrival of these dates, all Civil and Military Officers are directed each and all to save and guard (the Sun and the Moon).

(2) *The Proclamation for Saving the Eclipses of Sun and Moon.*

On the 10th day of the ninth Moon (21st Oct., 1901). The "Sū-Tsūng Tai Ngok Gi" (title) Toa Toi "Un" (surname) has given proclamation concerning that the eclipse of the Moon will take place on the 15th day of the ninth Moon "Mo Yan" (date's name) or (26th Oct., 1901), and eclipse of the Sun on the 1st day of the tenth Moon "Kwai Chi" (date's name) or (11th Nov., 1901). During the festival of the above dates all the Civil and Military Mandarins should be saved at the same time.

Two instances of the way in which novel-writers use the stars as scenic effects strongly contrast in carefulness with regard to facts. The first is in Marion Crawford's novel, 'Mr. Isaacs,' where the hero points to Sirius as his star *in the south*, the time being 9 P.M. in September 1879, when Sirius souths about 6 A.M. It should be remarked, however, that in September 1879 Jupiter was about the place indicated, and this may be the explanation of the mistake.

The other writer, Lucas Malet, may be congratulated upon her accuracy of description. In 'The History of Sir Richard Calmady' (p. 191) she thus describes a "windless autumn night":—

> The night was very clear. The stars were innumerable—fierce, cold points of pulsing light. Orion's jewelled belt and sword flung wide against the blue-black vault. Cassiopeia seated majestic in her golden chair. Northward above the walled gardens the Bear pointing to the diamond flashing of the Pole Star; while all across high heaven, dusty with incalculable myriads of worlds, stretched the awful and mysterious highroad of the Milky Way.

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No. 317.] APRIL, 1902. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 March 14.

Dr. J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the last ordinary Meeting were read and confirmed.

Mr. Whittaker. 110 presents have been received since the last Meeting. Among these, special attention may be called to a volume of the Professional Papers of the Survey of India Department, No. 5, which contains a full account of the investigations leading up to the question of the attraction of the Himalaya Mountains, which recently formed the subject of a paper by Major Burrard read to the Society, and also to a reduction by Dr. Auwers of the mean places of 570 stars to equinox 1815, obtained from observations made under Pond's direction in the years 1815 to 1819 at Greenwich.

Mr. Dyson read extracts from a paper by *Mr. S. A. Mitchell* on the Flash-spectrum as observed at Sumatra during the Solar Eclipse of 1901. He said that the conclusions to which Mr. Mitchell came were almost identical with those of Mr. Evershed, namely, that the flash-spectrum does really represent the upper more diffused portion of a stratum of gas which by its absorption produces the Fraunhofer spectrum.

Major Hills. So far as I can see from the photograph, the general character of the spectrum is very much the same as that taken on previous occasions. Of course, I have only heard bits of the paper, but from these it appears that the author says that he finds about half the lines of the solar spectrum at present in the flash. It seems to me that the comparison should be the other way—how many lines in the flash are in the solar spectrum. The solar spectrum lines are brought out under different measures of exposure.

Mr. Dyson then read further extracts from the paper.

Major Hills. Well, that coincides with Mr. Evershed's results. Does he mention anything about the presence of any particular metals?

Mr. Dyson. Yes, he mentions several.

Mr. Fowler. I think we should not accept the conclusion arrived at in this paper without very careful consideration. In conjunction with Sir Norman Lockyer I have had a good deal to do with the flash-spectrum and I am certainly not prepared to admit that the vapours which produce it are identical with those which produce the Fraunhofer lines. It cannot be denied that there is a reasonable agreement in the positions of the two sets of lines, but it is quite another story when the relative intensities are considered. Many of the lines which are weak in the dark-line spectrum are strong in the bright-line one, and it almost invariably happens that these abnormal lines in the case of such an element as iron are lines which are brighter in the spark than in the arc spectrum—that is, they are what Sir Norman Lockyer calls enhanced lines. This occurs too frequently to be a mere matter of chance. Yet, if I understand rightly, Mr. Mitchell follows Mr. Evershed in attempting to reconcile the two sets of lines by a process of averaging their intensities. This is most objectionable, as the significance of many of the most important lines is entirely destroyed.

It would seem that the absorbing vapours which produce the Fraunhofer lines are in a state which approximates to that of the arc, while the radiating vapours of the flash are more closely represented by the spark, the difference doubtless being one of temperature. The same vapour cannot be in two states at the same time, and one therefore argues that the layer of vapours which produces the bright-line spectrum is not the seat of the Fraunhofer absorption. At all events, a discussion of the flash-spectrum which takes no account of the enhanced lines cannot be regarded as final, and I think that if Mr. Mitchell had considered these lines he would have modified the conclusion at which he has arrived.

Major Hills. There is one point which I do not understand about Mr. Fowler's explanation, and that is, if the Fraunhofer absorption occurs below the region that gives the flash-spectrum, how is it that the latter shows a higher temperature? It is difficult to believe that the flash-spectrum region is the hottest portion of the Sun's envelope.

Mr. Fowler. If we suppose that the spark is at a higher temperature than the arc, as is probable, the vapours which give the Fraunhofer lines will be at a lower temperature than the flash-stratum. It therefore seems necessary to suppose that the absorption takes place above the stratum, where it is probably cooler. But since there is a condensation surface—the photosphere—beneath the stratum, it is not inconceivable that the necessary lower temperature may be found there.

Mr. Dyson then read a paper by *Mr. E. E. Barnard* on observations of Nova Cygni (1876) made with the 40-inch refractor of the Yerkes Observatory. Mr. Barnard had recently been measuring the distance of Nova Cygni from neighbouring stars.

Nova Cygni was now estimated as of magnitude 15·6, which was extremely faint, and seemed to show no movement since its discovery in 1886. Prof. Burnham had also made a series of observations of the magnitude, but did not come to any definite conclusion. Mr. Barnard concluded that the star was not variable. That was the substance of the paper, which was one rather to be read when in print than at a meeting of the Society.

Mr. W. H. Maw presented a paper on double-star observations, 1899–1901. He said that there were about 300 measures, and the paper was a continuation of a previous one. He regretted that there were not more measures, but that must be put down to the weather rather than to his desire to shirk work.

Mr. Lewis. Mr. Maw has said so little about his paper, that I should like to add my appreciation of the excellence of his observations. It is a great pleasure to find someone taking such an interest in the work of double-star observation.

The President called upon the Astronomer Royal to speak about several papers sent from the Royal Observatory, Greenwich.

The Astronomer Royal. It may be of interest to mention that in the course of measuring for the Astrographic Catalogue, carried out under the supervision of Mr. Hollis, he and his staff have found several variable stars. Three of them appear to be new, and another is U Draconis, whose variability was discovered by Dr. Anderson. We thought it worth while to give out this information so that anyone interested in variable stars can make use of the data. There is another paper, on the measures of double stars made at the Royal Observatory in 1901 by Mr. Lewis, Mr. Bryant, and Mr. Bowyer. There are two or three stars of special interest. The observations of Capella, of which the earlier measures were communicated to the Society at the end of 1900, have been continued, but the weather in the latter half of 1900 was so unfavourable that hardly any measures could be obtained. Since then Mr. Lewis and Mr. Bryant have succeeded in getting some measures lately which seem to be trustworthy. There is another star in which Mr. Lewis has taken special interest, namely δ Equulei. It has been observed at the Lick Observatory and at Greenwich under conditions in which it was very difficult to detect any elongation, and I think the results obtained at the two Observatories show very satisfactory agreement.

The President. I will ask Prof. Turner if he has any remarks to make upon the subject.

Prof. Turner. I should like to express my interest in the fact that in connection with the work on the Astrographic Chart some new variables have been discovered. The Greenwich method is to combine the scrutiny of two plates at the same time. They have a duplex micrometer to hold two plates which overlap, and so there is an opportunity for recognizing when a star is either not on one plate or is perhaps a little different in position or in brightness. A small difference in brightness may not be easy to detect because of the varying character of the two plates taken on different nights, though a striking difference in brightness can even then be detected. But some more rapid method of comparing plates seems to be urgently needed. The method used at Greenwich is of a laborious kind. It is doubtless within the knowledge of Fellows that there are other methods of scrutiny: and a representation of one of the latest devices, viz. the Stereo-comparator of Messrs. Zeiss, I will show on the screen, because it may be some little time before we shall have an opportunity of seeing the apparatus in England. Dr. Max Wolf, who has tried the instrument, has, I understand, approved of it. I think some method of this kind is needed for the detection of peculiar objects, for otherwise if we have to deal separately with each one of the millions of stars photographed for the Astrographic Chart, it will take some time and trouble. I should like to take this opportunity of mentioning a different sort of device which has occurred to me. Supposing we could throw upon the screen the images of two plates of the same region by means of two lanterns, so that we could make the images coincide star for star, and then cut off the light of one lantern, and after that the light of the other, alternately. If the correspondence of the two pictures was exact we should get a steady image in each case, but if there was any disparity for one star we should get some slight motion in the image of that star, and it appears to me that the sensitiveness of the eye to slight motion in one object when others are still might be used in the same way as is the sensitiveness to distance in the stereo-comparator; and although the method is not worked out in any way, I think it would be to the advantage of astronomy if methods of that sort were thought of and tried for comparison of plates.

The Astronomer Royal. Is the stereo-comparator intended for two plates taken with the same centre, or for plates generally which may only overlap partially, like those of the Astrographic Chart?

Prof. Turner. The instrument was originally made simply for two plates with comparatively large overlapping, but Messrs. Zeiss signify their willingness to adapt the instrument for a slight overlapping. One point about our astrographic plates which is more doubtful is with regard to those which have three images on, when it may be difficult to use the stereo-comparator, for the three images on the plate would complicate matters. It would be unexpected if the instrument turned out utilisable in that case.

Messrs. Zeiss are doing what they can to adapt it to wide conditions, and it is those improvements which make it impossible for them to tell us when they can let us have the first instrument, or what the price will be.

Mr. Lewis described in detail some facts about the determination of the orbit of δ Equulei.

The Astronomer Royal. Since the beginning of the year a number of photographs have been taken at Greenwich of the satellites of Neptune, and I thought it might be of interest to show the Society the results that have been obtained. An exposure of 20 min. is required to show the satellite well, while an exposure of 20 secs. or thereabouts shows Neptune. These relative lengths of exposure are given by means of an occulting shutter, which obscures the planet when necessary, and, as may be seen in the picture now on the screen, the satellite is photographed without being obscured by the image of Neptune. Special attention has been given to Neptune's satellite at Prof. Newcomb's request, who wished to have measures from these photographs, as he is determining the position of the satellite.

The Astronomer Royal, presenting the results of the Sun-spot record made at Greenwich during the year 1901, called special attention to a large spot seen at the time of the Solar Eclipse in May, at a point of the Sun's limb near which there was an apparent arch of the corona.

Mr. Maunder. The Sun during the past year, 1901, was extraordinarily quiet. The mean daily spot area for the entire year was only 29 as against 78 at the last minimum, whilst the record of faculæ was even smaller still, the smallest record we have got in any year since the Greenwich record was started. There were no fewer than 289 days out of the 365 on which no spots were seen at Greenwich. I have some slides with regard to the great spot of May 19th, the one which was seen immediately after the eclipse. The Astronomer Royal has pointed out that the place where the spot was on May 18 shows a large protuberance of the corona. The photograph now on the screen, which shows this very well, was taken 7 secs. after totality had begun, and was given almost an instantaneous exposure. The disturbance is seen very clearly both on the Sumatra and the Mauritius photographs. At first sight it looks as if there had been considerable change, but when you allow for the change in the position of the Moon, it becomes very difficult to be perfectly certain that any distinct change has taken place in the corona.

The President. I am sure we thank Mr. Maunder very much for the exhibition of these slides, and I will now ask Mr. Dyson to give an account of the paper from the Royal Observatory which relates to the proper motion of Nova Persei.

Mr. Dyson said that photographs had been taken since the Nova appeared except between May and August, and a number of these had been measured. The distance of the Nova was measured from 4 or 5 stars around it. The result was not altogether satisfactory, as it showed for the Nova a negative parallax with respect to the stars, but it is quite evident that the absolute parallax cannot be greater than $0''\cdot1$. The measures also showed an annual proper motion of the Nova of $0^{s}\cdot009$ in R.A. and $-0''\cdot20$ in Dec.

Mr. H. C. Plummer then read a paper on the images formed by a parabolic mirror—first paper, the Geometrical Theory. He said that his attention had been directed to the geometrical theory, and he found that it was scarcely in a state that could be called satisfactory. There were discrepancies which required to be cleared up, and in this paper he had tried to do so. The first paper in which the subject was seriously discussed was by Gen. Tennant in 1887, at the time when the relative merits of the reflector and refractor for the purpose of the Astrographic Chart were in question. After that the subject was left alone, and the decision was taken to use refractors, until 1897 or 1898, when the Crossley reflector which had been presented to the Lick Observatory failed to come up to expectation. Mr. Schaeberle attempted to explain the failure by a purely geometrical theory of the aberration. A paper by Mr. Crockett was published shortly afterwards, and there was a further paper by himself (Mr. Plummer) which he contributed to the *Astronomical Journal.* The method upon which all these investigations had been conducted had been to investigate the figure in which the rays incident on a circular ring of the mirror intersected the focal plane after reflection, and where the figures were given it was easy to see at once that discrepancies existed. In addition to the critical review of previous papers on the subject, the present paper included an extension of the theory so as to consider the images which would be formed on a plane photographic plate placed within the focus of the mirror or on a curved plate. The result seemed to show that little advantage could be expected under any circumstances from either plan.

Prof. Turner. I think Mr. Plummer has worked very hard on this question, which does not, perhaps, involve mathematics of a very high order, but is of a troublesome kind, and it is one which has been left in a very unsatisfactory state up to the present moment, especially as regards the working inside the focal plane.

The President. The first paper is on the geometrical theory, what will the subsequent work relate to?

Mr. Plummer. As I mentioned, I am actually at work on the measurement of a plate taken with a reflector, and a comparison of the results with those obtained from a photograph of the same region of the sky taken with a refractor of the Astrographic type.

The President. We have also a mathematical paper by our Secretary, Mr. Whittaker, and a paper by Dr. Alex. Roberts of Lovedale, South Africa, on the variation of S Carinæ.

Mr. Whittaker, referring to Dr. Roberts's paper, said the variability of S Carinæ was originally discovered by Dr. Gould at Cordoba, the period being then announced as several months. Since 1891 the star had been under observation at Lovedale, S. Africa, and Dr. Roberts had succeeded in finding the elements which define the variation. The chief points of interest were:—1st, that there was a secondary maximum coming before the chief maximum; 2nd, that the period (148 days) was subject to a variation, which could be represented by a trigonometric term with a period of 19 years; and 3rd, that the time of increasing brightness was nearly equal to the time of decreasing brightness; this was evidence against the supposition that with long-period variables there was a rapid rise to the maximum, followed by a slow decrease to the minimum.

Mr. Whittaker then gave an account of his own paper, which was concerned with the periodic solutions of the Restricted Problem of Three Bodies. In this problem the motion of a small planet is considered under the attraction of the Sun and a large planet, the latter bodies being supposed to move in circular orbits. Two theorems relating to the motion were communicated.

Mr. Dyson read a paper by *Mr. H. G. Fourcade*, communicated by Sir David Gill, on the method of surveying by photography, which Prof. Turner had lately brought before the Society.

Prof. Turner. I am glad to know that this method has been in use by Mr. Fourcade for some time, especially as Major Hills assured me, when I read the paper, that it was not likely to be used at all. I do not think we shall quarrel about the question of priority. The matter has been in my mind for more than a year, and I gave a sketch of it at the Photographic Convention in Oxford last July. Like Mr. Fourcade I was trying to find time to work it out in detail, but other things interfered. I hope, however, that Mr. Fourcade will not wish to take away all the credit of my little paper, because I think I may say it has attracted more attention than any paper I ever wrote. I have not only had this letter from Sir David Gill, but also two letters from America, to which I have written long replies, and I have also had one or two letters, not asking questions, but expressing admiration of the method. I mention this to show that however much the method may have been in use in South Africa it certainly is not well known and in use amongst people who want something of the kind. I have one letter from a gentleman accustomed to surveying in a mountainous district. He has already made trials of the new method, and the only thing he wants is a proper measuring-machine. I said little or nothing on this head because whatever would do for astronomical photographs would do for these surveying photographs. What remains to be done is to put our astronomical micrometers into the hands of those who require to use the method. I welcome the signs that we are making headway in our war against using angles when we are dealing with plane geometry, as on a photographic plate. I think this paper of Mr. Fourcade's will be welcomed by all who are interested in this matter.

Major Hills. I never heard of Mr. Fourcade before, and I must take his own word for the use of this thing. What I said was that I did not think it was likely that this method would be used to any practical extent by a skilled surveyor. I think it is possible that a traveller here and there may use it, especially a man who has not been brought up to survey matters, because this plan appears much simpler, but I think it is more a matter of appearance and that it will take just as long. I know that that is the experience in the Canadian Survey department, and they think there is no great advantage to be gained by it.

The President. It says here "this method has been used in its simple form." Does that mean that he used the camera?

Prof. Turner said perhaps it might be interesting to read the letter sent by Sir David Gill, dated February 11, with reference to Mr. Fourcade's paper, which is as follows:—

> I am sending to Wesley a note by Mr. Fourcade on your survey matter. Fourcade has a far more complete system than yours, viz., besides photographs taken from two points in horizontal directions at right angles to the line joining these points, he has réseaux on the plates, and he measures his x and y *stereoscopically*, with micrometers attached to both eyepieces. When the wires are one as seen with both eyes you are certainly seeing the same point with both eyes and get hence x, y, and z—from the horizontal and vertical microscope readings of both eyes.
>
> You can thus plot the plan and contour lines of any country from two photographs.
>
> Fourcade's paper was communicated to the S. A. Phil. Soc. in October last. When the paper is printed I will send you a copy. *It anticipates all you have said, and goes far beyond you.* * (Laughter.)

Major Hills. I feel certain from that letter that this gentleman has not practically used the method.

The President. But it has been used by him for some years and it is new to us.

Major Hills. But it has not produced any result.

The President. We will thank Mr. Fourcade for his paper, and Sir David Gill for his expression of opinion.

The following papers were announced and partly read:—

T. W. Backhouse. "Note on the Green Flash."

S. A. Mitchell. "The Flash-Spectrum, Sumatra Eclipse, 1901."

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* [The italics are mine and represent the emphasis with which these words were read at the meeting. I trust, however, that Mr. Fourcade will not think that the amusement caused by Sir David Gill's frankness extended to the subject matter of his paper. I should like to express my appreciation of his stereoscopic method of measurement, which seems to me admirably efficient, and had never entered my head.—H. H. T.]

E. E. Barnard. "Observations of Nova Cygni (1876) made with the 40-inch Refractor of the Yerkes Observatory."

G. W. Hough. "Anomalous Occultations of Stars by the Moon."

C. T. Whitmell. "The Duration of Totality at Navalmoral, 1900."

H. G. Fourcade. "Note on Prof. Turner's recent Paper on Photographic Surveying."

R. T. A. Innes. "The Magnitude of η Argûs, 1900–1902."

A. W. Roberts. "On the Variation of S Carinæ."

Rev. T. E. Espin. "Σ 484 and 485, and two pairs."

W. H. Maw. "Double-star Observations, 1899–1901."

Royal Observatory, Greenwich. "New Variable Stars found during the Measurements of the Astrographic Catalogue."

Royal Observatory, Greenwich. "Results of Micrometer Measures of Double Stars made with the 28-in. Refractor."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1901."

Royal Observatory, Greenwich. "Proper Motion of Nova Persei."

H. C. Plummer. "On the Images formed by a Parabolic Mirror. First paper : The Geometrical Theory."

E. T. Whittaker. "On Periodic Orbits in the Restricted Problem of Three Bodies."

The following gentlemen were elected Fellows of the Society:—

J. W. L. Child, Vernham, Merton Hall Road, Wimbledon.

J. Comas Solá, S. Felipe, 29 S. Gervasio, Barcelona, Spain.

Rev. J. H. Haslam, M.A., St. Saviour's Vicarage, Denmark Park, S.E.

Joseph Lunt, B.Sc., F.I.C., Royal Observatory, Cape of Good Hope.

J. Chandra Ray, M.A., Professor of Physical Science, Cuttack, India.

The following Candidates were proposed for election as Fellows of the Society:—

A. E. Conrady, F.R.M.S., 104 Park Street, Camden Town, N.W. (proposed by Dr. E. J. Spitta).

H. T. Gerrans, M.A., Worcester College, Oxford (proposed by Dr. A. A. Rambaut).

M. E. J. Gheury, 23 Downshire Hill, Hampstead, N.W. (proposed by Prof. S. P. Thompson).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE fourth ordinary Meeting of the twelfth Session of the British Astronomical Association was held at Sion College, on Wednesday, February 26th, *Mr. G. M. Seabroke*, President, in the Chair.

The names of five Candidates for Membership were read and passed for suspension and the election by the Council of four new Members was confirmed.

Mr. E. Walter Maunder, at the request of the President, made a statement with reference to the proposed erection of an observatory. He reminded the Meeting that a few years ago the Royal Botanical Society had offered the Association a site for the erection of an observatory in their gardens in Regent's Park, and that at the time a fund had been started for the purpose which now stood at about £159. The matter had been in abeyance for some considerable time, but the Council now felt that the financial responsibility of erecting and maintaining a suitable building would be greater than they were justified in incurring; they had accordingly declined the Royal Botanical Society's offer. The Council were, however, strongly of opinion that the idea should not be entirely abandoned, and recommended that the fund be retained for future use should a favourable opportunity occur. Members would remember that the late highly esteemed Director of the Solar Section, Miss Brown, had bequeathed one of her observatories with its contents to the Association. The telescope was in use by a member of the Association, but there remained the clock and the observatory itself, which the Council was anxious to see put to good service.

The President said that it was desirable that the observatory should be sold, but the clock would be on loan only.

Mr. Maunder drew attention to an exceedingly interesting memoir recently published by Prof. Brédichin, the Director of the Pulkowa Observatory, on the shape of the tail of the great comet of 1901. During their stay in Mauritius, Mrs. Maunder and he had had the opportunity of seeing this magnificent object, the most brilliant comet which had appeared since the autumn of 1882. They first saw it on May 3, and had photographed it on May 4 with the telescope so kindly lent them by Mr. Newbegin, and on the 6th May with Mrs. Maunder's little stigmatic lens of 1½ in. aperture. Two slides of this photograph were shown, the second being much enlarged from the first. Four photographs were also shown that had been taken at the Royal Observatory, Cape of Good Hope. It was principally upon these last photographs, Mr. Maunder said, that Prof. Brédichin's memoir was based. Many years ago he had made the forms of cometary tails his special study and had found that they corresponded to the forms which would be produced if a strong repulsive force situated in the Sun were acting upon the matter of which the comet was composed. He further found that cometary tails fell into three orders: first, those in which the effective repulsive force was something like twelve times as great as the force of gravity; second, those in which it was about equal to gravity; and third, those in which it was but a fraction of the force of gravity. Further, if it was assumed that the lightest element known at present—that is to say, hydrogen—formed the substance of tails of the first type, then hydrocarbons, the presence of which has been recognized by the spectroscope in comets on many occasions, might well, from their relative atomic weights, be large components of tails of the second type, whilst for the third type we should expect that the materials forming them would be heavy metals, such as iron. The present comet showed several features of great interest. On April 24, before its perihelion passage, the comet showed practically but a single tail, and that of the first type. After perihelion the tails were only of the second and third types, the matter composing the first type having been apparently exhausted. The Cape drawing of May 12 was of especial interest; it showed the bright tail of May 6 with its dark rift, a broad long faint tail of 25° in length, and a short tail between the two. The first of those, which might for distinction be called the principal tail, was due to a repulsive force a little greater than unity, and its two arms marked the anterior and posterior edges of a conoid of the second type. The broad diffused tail represented substances of the third type. A striking feature of this portion of the tail was that, when the particles were traced back to the nucleus, they indicated that a great explosion had taken place on April 22. A well-known and very striking example of cometary tails formed by a succession of explosions was that of De Cheseaux, of 1744, which showed no fewer than five great appendages radiating like a huge fan.

The Rev. E. Ledger remarked upon the enormous length of the third type of tail, and asked how far Mr. Maunder had followed it in the negative.

Mr. A. C. D. Crommelin stated that Major Eddie had traced the faint tail for nearly 40°.

Mr. Maunder said that the big broad tail, as he and his friends saw it in Mauritius, was certainly over 30°, even in the strong twilight they had at the time. It was indeed of the third type, though so much longer than that of the second type. It was represented in the drawing as straight, but in reality it was a small portion of a circle and represented the summation of a great number of emissions of varying atomic weights which took place at the explosion of April 22.

Mr. W. T. Lynn mentioned that it was seen at Paraguay on April 20.

Mr. Crommelin said it was seen about the middle of April, which was several days before other announcements. The discoverer's name was Viscara.

A Member inquired whether the two branches of the tail were two distinct formations.

Mr. Maunder said that Prof. Brédichin spoke of the principal tail as a single formation, the two branches on either side of the rift being the two faces of the same conoid.

Mr. Crommelin read a paper, by the *Rev. J. T. W. Claridge*, on "The Zodiacal Light." The writer considered that of all the forms which light took, the zodiacal light was the most delicately beautiful. More shadowy than the Milky Way, more translucent than the filmy nebulæ, it most closely resembled the aurora borealis, with which it had been frequently associated. The light shot up into the heavens soon after the Sun went down about the time of the Vernal equinox, and it also preceded the Sun when rising in the autumn. For all practical purposes it might be said that its axis coincided with the plane of the ecliptic on the Sun's equator. To an observer in England, the cone of light leaned, so to speak, somewhat to the left along the line of the zodiac. Hence the origin of the name given by Cassini in 1683. Kepler thought the zodiacal light was the atmosphere of the Sun. Cassini concluded the cause to be a solar emanation, and Mairan spoke of it as a solar reflection taking the form of a flattened spheroid. These views were opposed by Laplace on the ground that it would be impossible for the solar atmosphere to extend so far. Biot thought that the meteoric shower of 1833 was caused by the Earth passing through the material of the zodiacal light. The Rev. George Jones, who made over 300 successful observations in the Pacific Ocean, came to the conclusion that the zodiacal light was a ring of matter surrounding the Earth, not the Sun. Humboldt maintained that the zodiacal light was a very compressed ring of nebulous matter revolving freely in space between the orbits of Venus and Mars. Another idea was that the zodiacal light consisted of myriads of small meteoroids, each revolving round the Sun in its own path. It would seem certain, from the observed angular distance of the apex from the Sun, that these meteoroids, if such, extended beyond the Earth's orbit and that we must be constantly cutting through them as we revolved round the Sun. It was worthy of note that the zodiacal light had not been seen during a total eclipse of the Sun, indicating that its light was inferior in brightness to the corona. Spectroscopic evidence so far confirmed the theory that the light was mainly reflected sunlight.

Mr. Maunder said he had been very fortunate in seeing the zodiacal light on his voyage home from India, when it was unusually bright, its brightest portions being five or six times as bright as the Milky Way. He regarded the colour as a creamy white as contrasted with the steel-blue whiteness of the Milky Way. No satisfactory explanation had yet been given of the light. If they supposed it to be a ring of meteoric matter outside the orbit of the Earth, they were at once met with the difficulty that an exterior planet was brightest in opposition; consequently they would expect the zodiacal light to be brightest in opposition, which it obviously was not. It was brightest nearest the Sun. In reply to questions, Mr. Maunder said that the light had no sharp edge; it faded off quite gradually. When he was in the West Indies he kept a sharp look out for the zodiacal light, but never once saw it. Had it been even one-twentieth as bright as when he was in India he would have seen it.

Mr. Thwaites said that when in India he had seen the New Moon setting in the zodiacal light, and it seemed almost to put out the Moon's light.

Mr. Crommelin said that Mr. Evershed had thrown out the suggestion that the Gegenschein might be a sort of tail of the Earth, the lighter gases being driven off in the manner Dr. Johnstone Stoney had described.

Mr. Henry Ellis read a note on Mr. Gavin Burns' paper on the "Apparent Thinning-out of the Stars," in the last issue of the *Journal*, and papers by Mr. Gavin J. Burns on the "Light of the Sky," by Mr. Walter E. Besley on the "Fireball of 1902," and extracts from the report of the Director of the Meteoric Section were read, and the Meeting adjourned to 26th March.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, the 19th of March, at the Institution of Civil Engineers, *Mr. W. H. Dines*, President, in the chair.

Mr. W. N. Shaw, F.R.S., read a paper on "La Lune mange les Nuages," which was really a note on the thermal relations of floating clouds. He also exhibited an arrangement of apparatus whereby the conditions applicable in the case of a floating cloud can be experimentally realized.

Mr. F. J. Brodie read a paper on "The Prevalence of Gales on the Coasts of the British Islands during the 30 years 1871–1900." The total number of gales of all kinds dealt with during this period was 1455, the yearly average being 48·5, of which 10·6 were severe. The worst year was 1883, while the quietest was 1889. The stormiest month was January 1890. At all seasons of the year, excepting the summer, the prevalence of gales from south-west is greater than from any other quarter. The minimum of such gales is reached in the spring, when rather less than 20 per cent. are from south-west, more than half the storms being, however, from points between south-west and north-west. The prevalence of gales from polar directions is then at its maximum, more than 21 per cent. blowing from points between north and east; in the spring of 1883, out of a total of 11 gales no fewer than 7 were from these quarters, the proportion being about three times the average. The highest velocities recorded were those at Fleet-wood during the westerly gales on December 22nd, 1894, and on January 12th, 1899. On the former occasion for 9 hours, from 7 A.M. to 3 P.M., the mean velocity was 64 miles per hour, and at 9 A.M. it reached a maximum of 78 miles. It appears that on the average 43 per cent. of the storm systems which visit our coasts advance from some point of the compass lying between south and south-west and travel towards some point lying between north and north-east. 39 per cent. have an easterly motion, while less than 1 per cent. move westwards. A mean of 264 cases shows that the deep cyclonic systems which visit our islands travel on an average at the rate of 24·1 miles per hour; in some cases, however, the rate was not more than 8 or 10 miles, while in others it amounted to 40, 50, and even 60 miles per hour. The author concluded his paper by exhibiting a series of weather-maps showing the progress of some of the most notable gales during the period covered by the discussion.

Recent Determinations of the Sun's Movement in Space.

THE Sun's journey has a two-fold aspect. It may, in the first place, be regarded from the point of view of the human destinies bound up with it. To them it can scarcely fail to be of overwhelming importance. World-politics, evolutionary anticipations, the subjugation of nature by science, sink into insignificance by comparison with the momentous issues of the voyage upon which we are involuntarily engaged. The combined powers of our race avail nothing to check its speed or to divert its course; we are advancing, unconsulted and sub-consciously, towards an unimagined bourne. Just at present, the solar flotilla seems to be heading for the Milky Way. If steered straight, it might arrive there within some two million years; and the star-traffic in that crowded thoroughfare is evidently subject to accidents. But our path is likely to be otherwise directed. Suns can scarcely move from point to point as the crow flies. Indeed, the apex of their way is perhaps the spot in the heavens the most entirely remote from attainment by them. For they must inevitably pursue curved orbits, each body in the cosmos being swayed by the influence of all the others. They may, indeed, during a long span of time, continue to advance along sensibly rectilinear tracks; but deflections are sure, sooner or later, to become effective.

Then, again, sidereal science, in its largest bearings, depends essentially, for its promotion, upon acquaintance with the mode of travel of our system. Before we can attempt to pronounce how the stars are moving among themselves, we must be in a position to say how we are moving among them. Their flittings cannot even begin to be intelligible until they have been reduced, so to speak, to a fixed sun. Hence the zeal with which the subject is prosecuted. Every considerable addition to the store of determined proper motions gives a fresh stimulus to investigating energy. No pains are spared in working up the accumulated data. Scrutiny of their quality, siftings of their varied implications, improvements in the methods of their treatment, proceed indefatigably. The trend of error in the catalogues compared for the purpose of eliciting them is anxiously looked for; the value of the constant of precession is continually revised and refined; nor can the variation of latitude, with all its attendant complexities, be ignored.

What, so far, has been the upshot of this unremitting toil? Scarcely unanimity. Variously assorted collections of stars yield apexes far apart on the sphere. Thus, Kobold obliges the Sun to travel in the plane of the equator; Boss locates the *point de mire* of his journey halfway up towards the pole. Newcomb finds it to be in Lyra, Campbell in Hercules, Porter in Draco. Now discrepancies that spring from the reality of things are, in a manner, satisfactory, since they hold out the hope of increased knowledge; but these are not such. Their origin is subjective: they are symptoms of partial failure; they arise through defectiveness in the materials employed, or inability to discuss them adequately. Bakhuyzen, for instance, has pointed out that the anomaly of Kobold's result ensued from his treatment of "retrograde" stars, or those that make way against the current of perspective shiftings, owing to their being animated by "peculiar" motions agreeing with, but quicker than, that of the Sun. Other disagreements can be traced to the differently estimated influence of concerted stellar movements, of the kind seen to prevail in the constellations Orion and Ursa Major. Whether they should count as many, or only as one, is a question which has been variously answered by practical enquirers. It raises, indeed, considerations of a most arduous and far-reaching nature. If these "flying synods of worlds" form truly separate systems, each should obviously be assigned the weight of only one star. No sane computer would construct an equation of condition for every member of the Pleiad family. But the connexion subsisting between Rigel and Bellatrix, or between β and ζ Ursæ, may be totally diverse from that uniting Alcyone to her sisters. It may even cease to be perceptible, as minute inequalities, tending to separate the stars affected by them, accumulate with the slow efflux of time.

The Sun's apex, moreover, can be shifted hither and thither by the admission, or exclusion, from the research for its position, of stars with large proper motions. And here, plainly, individual judgment has a wide scope. The matter is one of degree, not of principle. A line has to be drawn, very much at discretion. By general agreement, a ban is placed upon visible "runaways"—upon 1830 Groombridge, upon the Ladas of the southern sky, Kapteyn's star in Pictor, and other racers; opinions, however, diverge as to the mode of dealing with less pronounced cases.

They present, indeed, something of a dilemma. The object in view is to disentangle the parallactic drift, which accurately reflects the solar motion, from disguising random displacements. But the element sought to be elicited, while fairly obliterated by large "peculiar" motions, is liable to emerge, through the exclusive study of sluggish objects, under an illusory or misleading aspect. The reason is obvious. They are disproportionately affected by observational inaccuracies. Near the limit of smallness, in fact, proper motions may just as well be simulated as genuine. No instrument is absolutely faultless; no theory of instrumental faults is absolutely complete; the eye, the ear, the touch of the observer are all fallible, and are often fallible, not indifferently but preferentially; and by the outstanding constant error, proper motions can with facility be created.

It follows that the judicious selection of an apex out of the multitude competing for acceptance is no light task. It demands, indeed, the possession of sagacity and critical insight, and their employment in patient and laborious enquiries. The desirable qualities, and the industry to use them, are happily combined in Prof. Kapteyn, and his work is thoroughly convincing as to the unwelcome prevalence of systematic errors in this difficult department of astronomy. Until lately, discrepant results were held by many to indicate actual differences of movement in specially selected bodies of stars. They were thought to afford a clue to dynamical relations on a stupendous scale. A possibility seemed discernible of separating the stars into vast companies, each obeying a distinct "stream of tendency." This is now perceived to be visionary. The imagined significant diversities originated, simply and solely, through the influence of methodical error.

Three determinations of the Sun's movement in space stand out from the rest as of particular interest and authority, namely—Newcomb's, Kapteyn's, and Campbell's. Newcomb* deduced an apex in R.A. 277°, D. +35°; Kapteyn † fixed the point in R.A. 274°, D. +29°; Campbell ‡, working with the radial velocities of 280 stars, found its coordinates to be R.A. 277°, D. +20°, the declination, however, being admittedly uncertain owing to the paucity of available materials from the southern hemisphere. That the solar system is at present travelling along a line directed about midway between α and β Lyræ, may, then, be admitted as probable. The right ascension of its goal appears, at any rate, to be established within moderate limits of probable error; while the high declinations assigned for it by Boss and Porter are certainly open to grave objections. Above all, the rate of our advance has at last come to be known, apart from dubious assumptions. Directly given by the spectroscopic method, it came out from Campbell's discussion 20 kilometres (12·4 miles) per second. Each year, accordingly, we explore a belt of space nearly 400 millions of miles in width, and our travelling, like that of clouds, is "irrevocable." Shall we find ourselves in an "ampler ether" as we proceed? Or will the wreckage of our little planet help to stock the void with meteorites?

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* *Astr. Journ.* No. 457.

† *Astr. Nach.* Nos. 3721–2.

‡ *Astrophysical Journal*, vol. xiii. p. 80.

> "It may be that the gulfs will wash us down;
> It may be we shall touch the Happy Isles."

Even the poets scarcely knew for certain which fate overtook Ulysses when he "sailed beyond the sunset" into a newer world.

AGNES M. CLERKE.

The Leonid Shower of 1901 *.

It now seems possible to give a brief review of the character of the Leonid shower which occurred last November, a considerable number of reports being available for the purpose. Certain other results obtained in various quarters of the globe doubtless still remain unpublished, but it is not likely that they will materially differ from those already before us.

In England the display of Leonids cannot be said to have been a conspicuous or a plentiful one, though it was decidedly stronger than in either of the years 1899 or 1900. Fortunately, the sky was clear on the nights following November 14 and 15, and a large number of observations were secured in different parts of the country. On the morning of November 15, Mr. H. Corder at Bridgwater watched the firmament for an hour and a half and saw 50 meteors, three-quarters of the number being Leonids, so that the horary rate of their apparition was about 25. Mr. E. C. Willis, of Norwich, found the hourly number 22 on the same morning, while on the following morning it was 18. Mr. J. R. Henry, of Dublin, observing from 1^h to $3^h\ 30^m$, saw Leonids as bright as the first and second magnitude falling at the rate of 12 per hour. The maximum was apparently attained just before the morning twilight began to overpower the fainter stars. The horary numbers quoted above may be fairly considered to approximate to the truth. It is true that some other observers saw fewer meteors, but as they were engaged in recording the individual paths, a considerable number must have altogether escaped their notice.

But the real maximum of the shower certainly occurred after sunrise in England; this is proved by observations from America, where the meteors were far more numerous, though the display was only of secondary importance. Mr. E. L. Larkin, at the observatory on Echo Mountain, S. California, counted 297 meteors on November 15, 4^h to 5^h A.M. (local time), so they were falling at the rate of 5 per minute. The maximum was at about $4^h\ 20^m$–

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* From *Nature*, 1901 Feb. 6.

25^{m} A.M. One fireball left a streak for 14 minutes, and the meteors generally were very brilliant, two being estimated twenty times as bright as Venus, three ten times as bright, twelve five times as bright, and twenty-five equal to Venus. Forty were equal to Jupiter. At Ladd Observatory, Providence, the number seen was 2 per minute on the morning of November 15, while at several other places in the States the rate was 3 or 4 per minute between about 3^{h} 30^{m} and 5^{h} 30^{m} A.M. At Carlton College, Northfield, Minn., it was estimated that four observers might have counted about 1600 meteors per hour. There was a marked falling off in numbers on the morning of November 16. Yet at two stations, according to newspaper reports, the shower was quite striking on the latter morning, for at Los Angeles one observer is said to have counted 385 meteors in the hour between 4^{h} and 5^{h} A.M., while at Phœnix 200 were seen in half an hour. It is highly probable, however, judging from the character of the shower as recorded at other stations, that in the two latter cases the observations were really made on the morning of the 15th and not on the 16th as stated in the newspaper accounts, which are often erroneous in such matters.

The maximum of the display must have occurred at about 11^{h} 30^{m} A.M. G.M.T. November 15, according to some of the best American descriptions. Possibly it may have been attained even later than this, for the morning twilight must have affected the observations to some extent. If the time of greatest frequency was after that stated, the phenomenon at its best could only have been observed from the Pacific Ocean, and it is not probable that we shall get any satisfactory reports from this region.

Though the shower was pretty active, it does not appear that photography has afforded any material assistance in recording its features. Plates were exposed at many observatories, but trails were absent upon them except in one or two isolated instances.

The place of the radiant found by Mr. Winslow Upton at the Ladd Observatory, Providence, was on November 15 A.M. $150\frac{3}{4}$ $+21\frac{1}{2}^{\circ}$, and on November 16 A.M., 151° $+21\frac{1}{4}^{\circ}$.

The next return of the Leonids will be regarded in an interesting light, for 1902 will afford the 1000th anniversary of the first record of the shower (902). The Moon will be full at the middle of November, but as the meteors of this swarm are often brilliant, some of them are likely to be distinguished in spite of the illuminated sky. There were showers of Leonids in 902, 1002, 1202, and 1602, and the revival of the display in 1901 encourages the hope that something may be seen of it in 1902, though the parent comet will be about three and a half years past its perihelion.

W. F. Denning.

Notes on some Stars in Lalande's Catalogue.

THE following notes on some stars in Lalande's Catalogue may be of interest to observers of variable stars. The positions are given for 1900·0.

Lalande 1013–14. R.A. $0^{h}\ 34^{s}{\cdot}4$, N. 51° 30′. Rated once as 10 m., and once 5 m. by Lalande. It is 7·7 in Argelander's D.M., 6 in Harding's Atlas. It is not given by Heis, nor is it in Fedorenko's Catalogue, or in Argelander's Zones. It was estimated 7·9 by Knott on Nov. 8 and 19, 1880. I found it fainter than 2 star *nf.* on Nov. 20, 1880, and Aug. 17, 1883.

Lalande 4098–9, 4100. R.A. $2^{h}\ 7^{m}{\cdot}7$, S. 2° 52′. Rated 9, 6½, and 6 by Lalande. It is 5·63 in the Harvard D.M.

Lalande 5573. R.A. $2^{h}\ 58^{m}{\cdot}0$, N. 56° 19′. Rated 8½ by Lalande, but measured 5·11 at Harvard. It may possibly be an Algol variable.

Lalande 6816–17. R.A. $3^{h}\ 40^{m}{\cdot}4$, N. 65° 13′. Rated 6 and 4½ by Lalande. Measured 4·68 at Harvard; the residuals of the measures are large in the H. D.M.

Lalande 13013. R.A. $6^{h}\ 40^{m}{\cdot}6$, N. 2° 37′. Rated 9 m. by Lalande, and 8 m. by Harding. I estimated it 7·6 m. on March 16, 1887.

Lalande 13072. R.A. $6^{h}\ 42^{m}{\cdot}6$, N. 3° 2′. Rated 7½ by Lalande and 7 m. by Harding, but I found it only 8½ m. on March 16, 1887.

Lalande 15096. R.A. $7^{h}\ 40^{m}{\cdot}8$, N. 12° 1′. 6 m. Lalande and 8 m. Harding. I estimated it 8¼ m. on April 8, 1886.

Lalande 30610. R.A. $16^{h}\ 42^{m}{\cdot}6$, N. 43° 17′. Rated 9 m. by Lalande and 8 by Harding, but I found it about 6½ m. on Oct. 16, 1889.

Lalande 33212. R.A. $19^{h}\ 17^{m}{\cdot}2$, N. 10° 44′. 7 m. Lalande and Harding; but I failed to see it with binocular on April 27, 1878.

Lalande 36860–62. R.A. $19^{h}\ 24^{m}{\cdot}3$, N. 24° 47′. 7, 6, and 7 Lalande, 6 m. Harding, and measured 7·26 at Harvard. I estimated it 8 m. on Oct. 13, 1888.

Lalande 38564. R.A. $20^{h}\ 2^{s}{\cdot}8$, N. 38° 48′. Rated 7 m. by Lalande, 6–7 by Argelander, and measured 7·03 at Harvard. But it is 9·4 on the Paris photographs. I estimated it 7·3 on August 29, 1886.

Lalande 39620. R.A. $20^{h}\ 29^{m}{\cdot}2$, S. 6° 11′. No magnitude given by Lalande, but it was rated 9 m. by Hencke, 9 by Harding, and 7·0 in the *Uranometria Argentina.* It was measured 8·52 at Harvard, and it is probably variable.

Dublin, 1902, March 13. J. E. GORE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Eclipse of Thales and that of B.C. 557.

GENTLEMEN,—

It seems to me that Mr. Monck, in his last letter to you, has missed the principal point that is novel in Prof. Stockwell's paper, which is to suggest that Thales was acquainted with a period of eclipses in 82 lunations, and that, having observed one in B.C. 610, on September 30, he predicted another at the next end of that period, on B.C. 603, May 18, whereas Mr. Monck says that Prof. Stockwell carries us back to the old date B.C. 610.

Our knowledge of the history and chronology of those times is scarcely sufficient to enable us to decide positively as to the date of the battle interrupted by the eclipse; but it does give a certain degree of probability to that of B.C. 585 over earlier dates, and especially over B.C. 610, and I cannot but think it very unlikely that Thales should have anticipated Prof. Stockwell in the discovery of the above cycle, which requires a longer record than was probably in the hands of the Greek astronomer. But when Mr. Monck goes on to contend that the eclipse which is sometimes said to have led to the capture of the Median city of Larissa by the Persians is the same as that which, according to Herodotus, put a stop to the war between the Medes and Lydians, and is commonly called the eclipse of Thales, I may be allowed to point out by how wide an interval, both of space and time, the two events were separated. The first was in some part of Asia Minor, probably in the vicinity of the river Halys; the latest date assigned for it (which I still believe to be the true one) was B.C. 585. The only account we have of the second is contained in the third book (cap. iv.) of the 'Anabasis.' There are difficulties connected with its circumstances, but the situation of Larissa (identified with "Nimrûd") is so described by Xenophon that there is no doubt whatever about its locality; it was on the Tigris, not far from Gaugamela, where Alexander overthrew the Persian Empire in the battle usually called that of Arbela, a place which is better known, but farther off. It is well known that Xenophon, in the 'Cyropædia,' represents Cyrus as having obtained the empire from the Medes by inheritance, in consequence of a marriage between his mother and Cyaxares, an apparently imaginary son of Astyages. But the transference appears to have really taken place as the result of a long war, which probably commenced about B.C. 559. The 'Cyropædia,' in fact, is only to be regarded a a sort of historical novel; and, in this place in the 'Anabasis,' Xenophon shows himself aware that a war had really been waged between the Persians and Medes. For the King of the Persians to whom he alludes must have been Cyrus; and though he uses the expression ὅτε παρὰ Μήδων τὴν ἀρχὴν ἐλάμβανον Πέρσαι, which is consistent with either mode of obtaining possession, the statement of a siege and the implication that the Median city, formerly large and strong, was ruined by the operations, clearly proves a state of war between the nations. Whether the cloud, which the historian says covered the Sun, and, inducing the besieged to retire from the fortifications, led to the Persian capture of the city, really refers to an eclipse is very uncertain. Mr. Johnson thinks it does, and that it was the eclipse of May 19, B.C. 557, which was total over the Mediterranean and in Syria. But, at any rate, the date was at least nearly thirty years after the battle described by Herodotus, of which Xenophon could hardly have been ignorant.

Let me conclude by remarking that, according to both Oppolzer's 'Canon der Finsternisse' and Ginzel's 'Spezieller Kanon der Sonnen- und Mondfinsternisse,' the central line of the eclipse of B.C. 557, May 19, passed some distance to the south of Nineveh and Nimrûd; but it was probably nearly total there, towards the end of its course. Prof. Stockwell proposes the eclipse of B.C. 547, Oct. 23, which would have passed to the north of Assyria, and would not have been total at Nineveh according to either Oppolzer or Ginzel. As I remarked in the *Observatory*, vol. vii. p. 381, one cannot help feeling, with Prof. Newcomb, that it is not likely that Xenophon, by the word νεφέλη, intended to refer to an eclipse; and I am glad to find Prof. Ginzel taking (somewhat doubtfully) the same view, *i. e.*: "dass von Xenophon eine meteorologische Erscheinung beschrieben wird." He adds that if the allusion be really to an eclipse, it is not one that can be used in improving the lunar theory.

Yours faithfully,

Blackheath, 1902, Mar. 8. W. T. LYNN.

The Reduction of Hornsby's Observations.

GENTLEMEN,—

It would seem only courteous to Dr. Rambaut to make a brief reply to his letter of last month, though I hope we need not protract the correspondence. The point at issue between us is perfectly definite and clear, viz., the fair price for the reduction of 80,000 transits and 20,000 zenith-distances. Dr. Rambaut puts it at £2000: I maintain that this is quite three times too much, according to prices which English astronomers are paying for such work. Dr. Rambaut quotes, in support of his view, the opinion of men of eminence, for whose opinion on some matters I have the greatest respect, but who have no experience of the matter in dispute, viz., the English price of computation. There is no need to mention their names unless Dr. Rambaut wishes; but the people who really know the proper figures, viz., the Astronomer Royal and the Greenwich staff, he took care not to consult. I have done so, and find their opinion more than confirms mine, for they make Dr. Rambaut's estimate more than *five times too much*! Hence I shall be again under the disagreeable necessity of opposing Dr. Rambaut's application to the Government Grant, which he has sent in again unmodified.

University Observatory, Oxford,
1902, March 5.

Yours faithfully,
H. H. Turner.

[In reply to the above, Dr. Rambaut writes to us under date March 18, as follows:—"I am much obliged to you for permitting me to see a proof of Prof. Turner's letter of March 5. In reference to it, I may say that I should welcome the opinion of any competent person who would take the trouble to examine the question. I have referred the matter to Dr. Downing, the Superintendent of the *Nautical Almanac*, who was for 19 years a member of the Greenwich staff, and who is thoroughly familiar with the Greenwich system. He has examined the records and enquired into each step of the work. His estimate slightly exceeds mine. I may add that I have written to the Astronomer Royal asking him to furnish me with data for forming an estimate of the cost of reduction at Greenwich."—Eds.]

Jupiter's Mass.

Gentlemen,—

I entirely agree with your note at the end of Prof. Sampson's letter in this month's number, that it would serve no useful purpose to prolong the discussion about this subject any longer. It has led to the detection of one mistake in Marth's work and my own, and for that reason it has been very welcome to me; but Prof. Sampson's latest note has convinced me that a continuation of it at present would not bring us any nearer to the truth. I may add that that note has not convinced me of anything else. I have replied at length to Prof. Sampson's former criticisms, and I have stated my reasons for treating the points in question as I did. So long as Prof. Sampson does not prove (which is a different thing from simply asserting) that these reasons are invalid, I think I can confidently leave it to competent readers, who may be interested in the question, to decide for themselves on which side the truth lies *.

Yours faithfully,

Gröningen, 1902, Feb. 25.

W. de Sitter.

The alleged Change of Colour in Sirius.

Gentlemen,—

Unless we are prepared to deny the authority of Seneca, it appears to me that we are bound to admit the evidence of a change of colour in an important first-magnitude star, although that star may not be Sirius. Horace's "rubra Canicula" should not carry much weight, for he probably took his astronomy at second-hand from Aratus; but Seneca's statement, "sed acrior sit Caniculæ rubor, Martis remissior," is categorical and appears to be made from personal observation. Moreover, since he is speaking of colour, it would seem impossible to adopt Mr. Lynn's conjectural emendation of "fulgor" for "rubor," as the sentence would then be pointless. That by Canicula is intended Procyon may well be; but in that case we still have the evidence of change of colour, for Procyon is no more red than is Sirius at the present time.

---

* For full details of the numerical reductions and discussions connected with the whole of my work, I may refer to vol. iii. part 1 of the *Cape Annals*, which will soon be published.

Yours faithfully,

Staines, 1902 March 13. FRANCIS GARE.

GENTLEMEN,—

Since Mr. Lynn did me the honour of mentioning my name in his last letter to *The Observatory*, I wish to emphasize the fact that Prof. Schiaparelli, in his *Rubra Canicula* papers, though giving unquestionable proof that, at first, among the Romans of the Augustine times, the word *Canicula* meant Procyon, as in Hyginus, Horace, Pliny, Cicero, &c., does not consider that *all* the Latin writers use the word in the same sense *, especially Seneca, who most probably means Sirius in his celebrated "acrior sit Caniculæ rubor, Martis remissior." Schiaparelli agrees entirely, as to the explanation of those words, with Mr. Lynn's opinion (*Observatory*, vol. x. p. 105) that "he [Seneca] may be speaking of certain occasions when atmospheric conditions would certainly give Sirius a reddish appearance."

But the fact that Procyon was sometimes spoken of as red by some authors, though giving no proof about the main point of the controversy, viz. the change of colour of Sirius, seems to diminish its probability, since we should then admit *two* of these strange occurrences instead of one; it is much easier to admit that *rubor*, *rubra*, &c. were used rather carelessly by the ancient writers in question, without a definite meaning about colour or an accurate personal observation.

The exhaustive discussion of so eminent a scholar and astronomer as Prof. Schiaparelli seems indeed to settle definitely the question in this sense. I remain, Gentlemen,

Yours truly,

Royal Observatory, Lisbon, Tapada,
1902, March 16. F. OOM.

Confirmed or New Variable Stars.

GENTLEMEN,—

Since my last communication on the above in your issue of May 1901, I can report the following orange stars as certainly variable, though they do not appear in Chandler's 'Third Catalogue of Variable Stars.' The list includes Birmingham 211 Cancri, announced in that letter as being brighter than usual.

---

* "Tuttavia é indubitabile che presso *molti* scrittori latini la parola Canicula rappresenta il Gran Cane o la sua maggior stella, Sirio" ('Rubra Canicula,' first paper, p. 23).

1. Es.-Bm.	2. Bm.	3.	4. Constellation.	5. R.A. 1900.	6. Decl. 1900.	7. Mag.	8.	Spe t
				h m s	° ′			
54	...	B.D. +58°·501	Cassiopeia	2 32 18	+59 10	9·5	...	I
97	83	+67·350	Camelopardus	4 40 51	+68 0	7·0	ROr$^{4\cdot1}$	I
101	85	+28·707	Taurus	4 45 15	+28 21	8·1	Or$^{3\cdot9}$	
152	120	+24·943	,,	5 39 6	+24 23	8·5	ROr$^{4\cdot0}$*	
158	123	+30·1014	Auriga	5 41 41	+30 36	8·5	ROr$^{3\cdot4}$†	
281	211	+17·1973	Cancer	8 49 45	+17 37	6·5	Or$^{4\cdot3}$	
696	...	+45·3349	Cygnus	20 54 32	+46 5	8·1	ROr$^{3\cdot7}$	

* Midway between ROr and Or.
† Midway between ROr and OrR.

Col. 7 gives the B.D. magnitude; col. 8 the average colour from my observations, the notation being the same as that in letter on p. 278, *The Observatory* for July 1897; col. 9 gives the type of spectrum as given by Krüger in his 'Catal. der farbigen Sterne.'

Es.-Bm. 54 is the same as Es.-Bm. 54*a*, there having been a mistake in the identification in forming the appendix to that catalogue. It is orange, but too faint to show colour well with a 4¼-inch refracting telescope.

So far as my observations have gone, the variation of all these stars, although certain, is small, none of them much passing a range of a half magnitude. That of Birmingham 85 is less than a half magnitude as occasionally observed during the last five years. Gore includes it in his list of suspected variables, showing a range among the various authorities of three magnitudes. Of course my observations do not prove that this range is not sometimes reached, especially as we know that this class of variable is apt to show occasional changes much greater than at other times.

In all cases the periods are long or irregular.

West Hendon House,
Sunderland, 1902, March 17.

T. W. BACKHOUSE.

An Eclipse Phenomenon.

GENTLEMEN,—

The mention of an observation of a ray of light projecting from the cusps of the Moon in the solar eclipse as having been seen in 1870 by the late Mr. Brett, calls my attention to the same phenomenon (apparently) having been noticed in 1868 by Captain Branfill and Major Hearne under similar circumstances.

Mr. Brett appears to have seen the lines only near totality in 1870, but Captain Branfill saw them when three-quarters of the Sun were eclipsed, and I see from Mr. Ranyard's volume of the R.A.S. Memoirs that they have been seen when only half the solar disc had vanished. It would seem, then, that these lines must be frequently present in partial eclipses, yet I do not recollect ever having seen them noticed.

The phenomenon seems not to be visible continuously, but we can hardly conceive that the totality of the eclipse can have anything to do with it, and the search for it and its cause would give an interest to the observation of partial eclipses such as they have not now. Have no such appearances been seen in making the micrometric measures of the cusps at Greenwich and elsewhere?

11 Clifton Gardens, Maida Hill, W. I am, yours faithfully,
1902, March 22. J. F. Tennant.

OBSERVATORIES.

Tacubaya, Mexico*.—*Meridian Circle.* This instrument is used for observations of standard stars for time, reference and guiding stars for astro-photographical work, comparison stars used in asteroid observation, and occasional work, such as stars needed for latitude by the Geodetic Commission and some other observers, and some longitude work.

The number of transits observed from July 1st, 1899, till the end of December, 1901, was 11,786, and the number of declinations 7684.

With the 15-inch equatorial, Eros was observed on 61 different days, its differences of Right Ascension and Declination from neighbouring stars being measured micrometrically.

These stars were, shortly afterwards, re-observed with the same instrument.

Astrographic Work. The photographic work in hand consists of the catalogue plates for the Astrographic Chart and measurement of coordinates of the stars contained thereon.

Prior to the readjustment and correction of the object-glass, by Alvan Clark & Sons, 591 field plates had been taken, as stated at the Conference held in Paris in July 1900.

After that important improvement, 156 new plates were taken

* [For this account of the work at Tacubaya we are indebted to the recently appointed Director, who has begun his directorship by taking a zone of the Astrographic Chart. We offer our congratulations on the result of his present labours and our best wishes for the future.—Eds.]

during the first half of 1900, and later on, up to January 31st, 1902, the full number of plates exposed amounted to 604, as follows:—

Catalogue Plates, 377; Eros Plates, 178; Oxford fields, Pleiades, Orientation Plates, &c., 49.

Of the Catalogue Plates, 97 were rejected as defective. Moreover, 125 additional plates demand a more careful examination, because of the defective nature of the emulsion used in them. Indeed, the emulsion proved to be very slow and of a heterogeneous composition.

As before stated, the total number of plates exposed, from January 1st, 1900, to January 31st, 1902, was 760.

I must also state that many of the recently exposed Catalogue plates have been taken, to substitute for some of the plates exposed before the adjustment of the object-glass. As, in most cases, the emulsion of French plates arrives utterly altered, owing, perhaps, to the long period of time it takes in reaching us, we have been compelled to make a trial with American plates; and for the most part they have proved satisfactory.

As regards plate-measurement, 173 been had measured in a single position, and 30 were measured in two positions, up to June 1900. Later on, 161 belonging to the Catalogue and 12 to Eros have been measured, both series in two positions. The full number, therefore, of plates measured in two positions was 191, comprising 41,148 stars.

As a basis for carrying out the reduction, standard coordinates have been computed for 38 plates.

Miscellaneous. Some time is devoted to the observation of small planets and comets, and meteorological observations also find a place in our Observatory.

The computations of the meridian observations are carried on as fast as possible; and we are at present re-computing the observations taken in 1900, so as to be able to publish the results as early as possible. FELIPE VALLE.

[Continued from p. 96.]

STOCKHOLM. *K. Bohlin.*—The re-observation of Radcliffe stars has been the work of the Transit Department. In the Astrographic section much time has been devoted to researches on the measuring micrometer.

STRASBURG. *Dr. Becker.*—The large refractor was in use for observations of Nebulæ, and Comets, and of Eros. With the Repsold meridian circle, observations were made of circumpolar stars between 60° North Declination and the Pole. The whole number of observations with the instrument, besides those of Polaris and δ Ursæ Minoris for instrumental error, are:—

Fundamental stars	1986
Circumpolars	1871
Eros stars	572
Stars for latitude variation	130

and about 150 observations of Moon, Mercury, Venus, Jupiter, Saturn, Uranus, Neptune, and Eros.

Utrecht. *Nÿland.*—Variable Stars, Leonids, Lyrids, Perseids; drawings of Jupiter; observations of comets, 1900. Time Service.

Wien. (*M. Edler v. Kuffner*) *L. de Ball.*—Zone observations. Measures of distance of Eros from two stars with the 6-inch heliometer. With the photographic refractor several negatives were obtained of the cluster G.C. 392. Installing transit for use in prime vertical.

Zurich. *A. Wolfer.*—The observations of prominence and of the physical features of the Sun were continued. Complete pictures of the Sun on 258 days, on only 58 of which are prominences. The observations of sun-spots were:—

1899. Relative No. 12·0. Days of observation 292. Free from spots on 93 days.
1900. „ „ 8·8. „ „ 276. „ „ 118 „

NOTES.

Minor Planet Notes.—The following new planets have been discovered by Prof. Wolf at Heidelberg:—

Designation.	Date.	Magnitude.
HR	March 3	14·5
HS	„ 3	13
HT	„ 3	12
HU	„ 4	12

Another planet HQ was announced, but this proved to be identical with (93) Minerva.

Photographic search was made for (428) Monachia, (392) Wilhelmina, but without success.

The following planets discovered last year have been numbered:—

Designation.	Date.	Number.	Designation.	Date.	Number.
GC..	1901 Feb. 13	473	GR..	1901 Aug. 23	477
GD..	„ „ 13	474	GU..	„ Sept. 21	478
HN..	„ Aug. 14	475	HJ..	„ Nov. 12	479
GQ.	„ „ 17	476			

476 has been named Hedwig.

GS is identical with (453), GT (perhaps) with (156) Xanthippe, GV with (202) Chryseis, GW with (247) Eukrate, HG with (455) Bruchsalia, HL with (358) Apollonia.

GX, GY, GZ, HA, HB, HC, HD, HE, HF, HH, HK were not sufficiently observed to receive permanent numbers.

A. C. D. C.

THE PROBABLE REVIVAL OF SOLAR ACTIVITY.—The first days of the past mouth afforded what may fairly be regarded as distinct evidence that the spot activity of the Sun has definitely passed its minimum. On March 3 a small but pretty group, forming a short stream, divided, as is so usual, into two clusters, passed the central meridian. On the same day one or two small faint spots were seen in the midst of a bright though small collection of faculæ near the N.E. limb. Both these groups were in high northern latitude, +25°; the first time for several years that two groups so far from the equator have been seen at once. The group near the N.E. limb on March 3 underwent considerable change as it approached the central meridian, and developed into a fine stream with an area of about 700 millionths of the visible hemisphere. Transit across the central meridian took place early in the morning of March 8. On March 9 the group was seen as a fine composite spot as leader followed by a pair of large spots side by side at a little distance. The leader was divided lengthways by a brilliant bridge, and transversely by one or two smaller ones. The only parallel to this group as to extent in 1901 was the one which came into view on May 19 immediately after the eclipse; but that group was situated in lat. +9°, and may therefore be considered as belonging to the equatorial zone and as a member of the expiring cycle. The two groups of the first week of March, on the other hand, from their higher latitude, are probably members of the cycle now beginning. It may be added that the magnetic records are beginning to show some slight disturbance.

Four groups in high latitude were noted in 1901; one of these, observed on September 12, being in lat. +36°. This was very small, and lasted but a few hours. Two of the other high latitude groups of the year were more considerable and were in the southern hemisphere. Their appearance in a year of such extreme quietude as 1901, followed, as it has now been, by a much greater outburst also in high latitude, is a strong indication that we have now definitely entered upon the period of revived solar activity.

The group which ran its course from Jan. 5 to Jan. 15 of the present year was in lat. −7°, and never attained the dimensions of the group described above.

E. W. M.

THE REPSOLD REGISTERING MICROMETER.—In connection with the remarks on Personal Equation in observing transits which appeared in last month's number, it may be mentioned that a plan for eliminating this error, which was proposed many years ago, has been realized, and is in active use at the Geodetic Institute, Potsdam, and also, it is believed, at Poulkova. This method, it is scarcely necessary to say, is for the observer to cause a movable wire to remain in contact with a star by means of a micrometer head, and this head makes electrical contacts at various points of its revolution, and so marks the chronograph-sheet. This device is mentioned in the *Monthly Notices R. A. S.* for 1864, where it is coupled with the name of Sir Charles Wheatstone, who may or may not have been the first inventor; but, at any rate, Messrs. Repsold have perfected the apparatus, and it now goes by their name. Herr Albrecht and his colleagues have used this micrometer for longitude work on a "broken" transit; that is, an instrument of the *coudé* form, the ocular being at one end of the axis of rotation. The telescope is, of course, reversible, and the observer, having followed the star for a short distance before the meridian so that five contacts are made, reverses the telescope in its bearings, and follows the star with the wire on the other side of the meridian, by turning the micrometer in the opposite sense and so making the same five contacts in the opposite direction. The mean of the ten times thus observed is, in theory, the time on a line at right angles to the axis of rotation, *i. e.*, on the line of collimation; and so it appears to be a very convenient and expeditious method; but it seems to us that if there is any variability in the line of collimation of the telescope—if, for instance, the telescope-tube had a flexure—it is altogether masked by taking this mean. In fact, the line of collimation is not exhibited. The results obtained by the method speak highly in its favour. In the determination of a difference of longitude Potsdam—Bucharest, by Profs. Albrecht and Borrass, the difference of their personal equation, which had been $0^s{\cdot}105$, was reduced to $0^s{\cdot}017$, and the accordance of results of individual night's work was remarkable, the mean error of the mean 22 nights being $\pm 0^s{\cdot}005$.

The re-determination of the difference of longitude between Paris and Greenwich is now proceeding. The observations are being made by two members of the staff of the Paris Observatory, MM. Renan and Bigourdan, and Messrs. Dyson and Hollis from Greenwich, the two observers of each nationality making an independent determination. The actual positions of the stations are two piers in the grounds of the National Observatory at Paris, and at Greenwich the Transit Pavilion, which is in Bradley's meridian, and a temporary pier erected in the Front Court. The first interchange of observers is about to take place (March 29). Mr. Dyson will return to Greenwich with M. Bigourdan, and M. Renan and Mr. Hollis go to Paris.

The next meeting of the Royal Astronomical Society will be on Friday, April 11, at 5 o'clock; of the British Astronomical Association on Wednesday, April 30.

FROM AN OXFORD NOTE-BOOK.

IT was only the other day that we welcomed a history of the Greenwich Observatory from the pen of Mr. E. W. Maunder—a short account of the popular kind, but, so far as I am aware, the only modern attempt at a comprehensive history of our national observatory. A history of the Paris Observatory *, which has just appeared, is built on a quite different plan: it follows the affairs of the observatory with considerable detail for the first century and a quarter only. This is not the place for a critical review of so important a work; but having spent the greater part of 7 hours' railway-travelling in looking through it, I will venture a remark or two. Under the particular conditions I found the first dozen chapters rather dull: the faithful account of details therein given is no doubt most valuable as a work of reference, but not exciting as literature. But Chap. XIII. (a lucky number this time) was quite different. When once the author has started on his real theme—the truth about the Cassini family, and the falsity of the accusations hitherto formulated against them—he grips the attention at once; and he never lets you go after that. The book increases in interest right up to the end, and the final chapter on the tragic expulsion of Cassini IV. at the time of the Revolution is a splendid climax. From Chap. XIII. to the end the book reads more like a drama than a scientific history! And yet M. Wolf loses none of his scientific accuracy in his desire to establish his point. There is no absolute whitewashing. In Chap. XIII. one's sympathies are aroused by the very difficulties of proving " the truth about Jean-Dominique Cassini." The author had convinced himself on the subject by a minute study which had reconstructed for him the man out of his ashes: but to reproduce the result for others in tangible form, rendered solid by complete evidence on each point, was another matter. In face of the obvious difficulties, he might have attempted to carry conviction by simple rhetoric: we feel grateful to him that he did not. There at any rate are the main facts, however meagre; and there alongside them is the perfectly definite opinion of M. Wolf (who has gone into the matter thoroughly) that J. D. Cassini was a great man who has been shamefully traduced. I venture to express the opinion that M. Wolf will get his verdict, and that in any case he has written a book well worth the great labour it has obviously cost him.

A FEW extracts from the book may be of interest. I will first give a specimen of the first twelve chapters, in support of my view that they are rather dull. Here is an anecdote of Halley (p. 116) (" plus intéressante "!):—

Plus intéressante pour lui fut la visite que lui fit Halley, après son retour de Sainte-Hélène lorsqu'il se rendait de Dantzig à Rome. Cassini le reçut à l'Observatoire, et l'amena à l'Académie le samedi 10 mai 1681.

" Cet astronome y fit son rapport de l'observation qu'il a faite dans l'Isle de

---

* ' Histoire de l'Observatoire de Paris, de sa fondation à 1793,' par C. Wolf. Gauthier Villars, 1902 (15 fr. or 15s.). 400 pages, 10 in. × 6½ in., and 15 plates.

Sainte-Hélène de Mercure dans le Soleil et de son Catalogue des principales étoiles australes" (Procès-Verbaux manuscrits de l'Académie Royale des Sciences, partie mathématique).

But when the author gets to Chap. XIII. the anecdotes are much better. Here is one, quoted from Bertrand it is true, but also the peg on which various remarks are hung by M. Wolf (p. 215) :—

> Un jour, une comète parut dans le Ciel. Le roi désira savoir vers quelle région elle se dirigeait. Cassini, qui ne l'avait observée qu'une fois, le lui dit immédiatement. La comète suivit une autre route, mais le Roi ne s'en informa pas et se souvint seulement que pour un homme aussi habile que M. Cassini les astres n'avaient pas de secrets.

On p. 255 there is an interesting view of the function of a national observatory, date 1785 :—

> Il serait bien utile qu'il y eût à l'Observatoire royal un cours complet et continuel d'observations astronomiques faites tant le jour que la nuit, depuis le commencement jusqu'à la fin de l'année afin que tous les astronomes, tant régnicoles qu'étrangers, les voyageurs et les navigateurs, pussent trouver dans les registres de l'Observatoire les observations correspondantes aux leurs, et celles même qui leur manqueraient et dont ils pourraient avoir besoin.

MILTON wrote (Paradise Lost, ii. 706–711) :—

On the other side
Incensed with indignation Satan stood
Unterrified, and like a comet burned
That fires the length of Ophiuchus huge
In th' Arctic sky, and from his horrid hair
Shakes pestilence and war.

Can any of those expert in such matters tell me whether this is a definite reference to the great comet of 1618? The lines were written about 1665, I suppose, and in Chambers's 'Handbook' I find big comets in 1618, 1652, 1661, 1664, 1665, of which only that of 1618 seems to fit the reference. Its tail was 104° long according to Longomontanus, and "firing the length of Ophiuchus huge" seems to be just what it did. In 1618 Milton was a boy of 10, and 40 years is a long time to remember a particular comet, and the name Ophiuchus. A friend whom I consulted asks "Was there an intermediate literary source, or was it a simple remembrance of childhood? I should half expect to find trace of some pamphlet with a title such as : 'A True Account of the Late Blazing Star, with some observations on the disasters to the Cause of Religion that ensu'd on its Appearance.'" There were disasters enough, for the year 1618 was the first year of the Thirty Years' War, which is an additional reason for identifying the reference with this particular comet. Did Milton hear the war (which followed a longish peace) ascribed to the comet by his elders and betters, and did the deep impression made on his youthful mind remain to be used in the lines quoted? I shall be glad of further light on the point, if any reader will be so kind. While examining it I found mention of a work, "The Astronomy of Milton's Paradise Lost, by Thomas N. Ochard, M.D." (Longmans, 1896. Price 15*s*.): a voluminous work of many pages, which I got hold of, only to find this luminous comment :—

It is not easy to imagine why Milton should have assigned the comet to this uninteresting constellation; he may possibly have seen one in this part of the sky, or his poetical ear may have perceived that the expression "Ophiuchus huge," which has about it a ponderous rhythm, was well adapted for the poetic description of a comet.

SEVERAL American correspondents have drawn attention to the fact that among the 137 "Captains of Industry" who gave a lunch on Feb. 26th to H.R.H. Prince Henry of Prussia were two well-known astronomers, Simon Newcomb and E. C. Pickering. It would appear from all accounts that it was a first-rate lunch: may we hope that the representatives of astronomy did it justice?

FROM the *Daily Telegraph*, date mislaid:—

> George Psalmanazar, as a first-class romancer, traveller, and inventor of languages, is now almost forgotten, although he was a well-known habitué of Fleet Street and boon companion of Dr. Johnson at the Cheshire Cheese, but his name was last night revived at the meeting of the Japan Society of London, when a paper on Formosa, prepared by Mr. James W. Davidson, U.S. Consul in that island, was read by Mr. Charles Holme. Psalmanazar was one of the most artistic fabricators that the world has ever seen; a thousand times cleverer than the gentlemen who invent the Continental stories about the Concentration Camps in South Africa, and compared with whom De Rougemont was an infant, and his "History of Formosa" would probably have been regarded as an authentic work until the Japanese went to the island had he not himself confessed the volume to be an imposture. One of his statements was that exactly at noon throughout the length and breadth of that country the Sun shone perpendicularly down each chimney with a dazzling glare. Since the Japanese took charge of the island its material and moral progress had been astonishing.

Is it, or is it not, fair to accuse the writer of the following passage of making the Sun throw light upwards, *i. e.*, of shining when it is below the horizon?

> The next evening found him in the nave of the Cathedral some time before the appointed hour for the lecture, for the verger had warned him that the attendance would be very large. The Sun was still shining warmly on the lime tree avenue outside, making the fresh foliage glow like a jewel of unearthly radiance in its blended gold and green translucence, throwing long powdery shafts of gold through the windows up into the dim recesses of the groined roof, and disclosing carven nooks only thus touched by the midsummer glory, and dark all the year long besides. ('The Silence of Dean Maitland,' by Maxwell Gray.)

LORD ROSEBERY *is* going to have a "sending" of astronomical references. Here is another from the *Contemporary Review* for March: Hector Macpherson on "the Real Lord Rosebery": first page:—

> During his brief Premiership, and since his retirement, Lord Rosebery, by his public utterances, has caused the greatest consternation among his political friends. Comet-like he sweeps across the sky, completely upsetting the calculations of political astronomers.

We get, moreover, a new view of comets; and if they should take to upsetting our calculations, they will cause more wonder than Lord Rosebery feared from an astronomical education, and perhaps more consternation than if they from their "horrid hair shook pestilence and war."

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 April 11.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. Amongst the presents received since our last Meeting and calling for special attention are:—The Lund Zone Catalogue, presented by the Astron. Gesellschaft—Conférence Astrophotographique International, Circular No. 9, presented by M. Loewy—Tachkent Observatory Publications No. 3, presented by the Observatory—F. C. Penrose, Method of predicting by graphical construction Occultations and Eclipses, 2nd edition, presented by the Author—8 Charts (Photographic Chart of the Heavens), presented by the San Fernando Observatory—4 lantern-slides of Leonid Meteor, &c., presented by Prof. J. K. Rees and Mr. C. A. Post—Astronomical drawings by the late John Brett, presented by Mrs. Brett.

The President. I will call upon Mr. Dyson to say a few words about the Circular resulting from the Astrophotographic Conference which met at Paris in July 1900.

Mr. Dyson said that, as Fellows might know, this Congress was called together for the purpose of organizing observations of Eros at Opposition for the determination of the solar parallax. Many observatories have taken part in it and a large number of photographs have already been obtained, and in addition a great number of meridian observations of stars have been made. The book which has just been received is the ninth circular which has been issued by M. Loewy on the subject; it is of great importance, as it supplies the means of reducing the photographs, and by the help of this circular those who took part in obtaining photographs of Eros may set to work at once and determine the solar parallax. The memoir contains meridian observations of the comparison stars made at 18 or 19 observatories; altogether 19,000 observations of R.A. and 16,000 of Declination have been made, giving an average of 29 and 24 observations respectively for each star.

M. Loewy has also had computed an ephemeris of Eros—a very complete one—by the use of 8-figure logarithms, a very troublesome piece of work and one which will be of great use. The elements of the orbit were supplied by Prof. Millosevich and the calculations were made at Paris under M. Loewy's direction.

M. Loewy draws particular attention to the necessity of those who have photographs of Eros measuring what he called the "comparison stars." He has given the names of "the reference stars" to those which have been observed by meridian instruments. The accurate positions of the "reference stars" are of importance for photographic observations, and absolutely essential for micrometric observations such as those made by Mr. Barnard at the Yerkes Observatory. Throughout the Opposition he consistently observed Eros whenever possible, measuring its distance and position-angle from neighbouring stars of the 10th to 11th mag. The Right Ascension and Declination of these stars are at present unknown, but can be obtained from the photographs, and it is very important that this should be done.

In addition to this there is an extensive memoir on the accuracy of measuring the star-images. This subject had been to some extent debated at the Society by several Fellows, and some of M. Loewy's previous conclusions have not been altogether accepted. In this memoir M. Loewy analyses the errors which arise in the deduction of stellar positions from photographs. He divides them into errors of measurement, intrinsic errors of the photographs themselves due to such causes as distortion of the film, and errors arising from incorrect value of the R.A. and Declination of the comparison stars. With regard to errors of measurement, one point which M. Loewy made was that the physiological error made in bisection (which is eliminated by reversing the plate) is very small. This error, according to M. Loewy, was very small. (Blackboard.) M. Loewy stated that the measurers at Paris were able to avoid this error by practice, so that the reversal of the plate showed no substantial systematic discordance.

M. Loewy found that the faint stars were measured with as much accuracy as the brighter stars, but owing to the non-uniformity in the sensitiveness of the film the images themselves did not represent the position of the star with as much accuracy. The measurer could readily get his results as accordant as with a bright star.

M. Loewy also draws attention to errors which seem to point to a small displacement of the gelatine in drying. He points out that a considerable error is introduced in the determination of the plate constant from the uncertainties in the position of the reference stars. Although the stars depend upon a large number of observations, the final error is quite as large as the error of measurement, or the errors arising from defects in the photo-film.

The President. I am sure we must all thank Mr. Dyson for having prepared for us so interesting an account of this circular.

Prof. Turner. I should like to say a word of thanks to M. Loewy for having prepared this ephemeris, which will be of the very greatest value, and for which those who have been working on Eros are much indebted to him.

Mr. Hinks. I am very glad to have the opportunity of adding a word to what Mr. Dyson and Prof. Turner have said of the value of the circulars issued by the President of the Eros Commission of the Paris Conference. In this combined attempt to determine the solar parallax by way of Eros, M. Loewy has been, not perhaps generalissimo—for that would imply a rigid discipline, and I fear that we have inherited from the makers of the astrographic chart a disposition to break resolutions when they are made—but "director of intelligence." He has charged himself with the duty of organizing the collection and distribution of all information that could be of use to the observers of Eros, and persuaded the Academy of Sciences to undertake the printing of circulars which run from three up to about two hundred pages. If I have made a plate at Cambridge and want to see if they have a corresponding plate at Lick or Minneapolis, I find every detail of information in the circulars. And there is one matter in particular for which some of us are especially grateful to M. Loewy. He has supervised the construction of an eight-figure ephemeris of the planet; and naturally he chose for it the canonical form, in apparent R.A. and Dec. But it has seemed to some of us that another form of ephemeris, in rectangular ecliptic coordinates, would be more useful in the photographic work; and M. Loewy very kindly and readily answered a troublesome request, and has given this form of ephemeris also. I am very glad that we have had the opportunity to-night of expressing our sense of the high value of these Paris circulars.

The President then called on Prof. Turner to give an account of two papers on the list under his name.

Professor Turner. One of the two papers which I have communicated to the Society, namely that "On Stationary Meteor-Radiants," does not call for any lengthy notice. Some time ago I read two papers developing the theory of stationary radiants on lines supposing the Earth's attraction of meteors to be the *vera causa* of the phenomenon known as stationary radiants. In connection with that theory it occurred to me we ought to consider the effect of the diffused atmosphere lying outside the Earth which would naturally be encountered by particles as they came near the Earth, and the present paper is devoted to a consideration of this effect; but it does not advance matters very much, although it does not retard them. I do not think I need trouble the meeting with an account of it, as it will be very much better understood by reading it.

The other paper will be of more general interest, dealing as it does with the simple observations of counting stars on a photographic plate, and with some very interesting results which follow; and I should like to explain it at some length. We adopted a plan at Oxford of counting the number of images on a plate, as a rough guide to its quality. We found sometimes after we had measured the images that the plate did not show enough stars, and, therefore, that there had been a waste of labour; and, of course, as measuring takes some considerable time, it was advisable, if possible, not to waste labour in that way. We found that a preliminary count took an hour per plate to enable us to say whether it should be measured or rejected. It will be familiar to the Fellows that the plate is crossed by a series of réseau lines. There are 26 of them in each co-ordinate. The counting is done by passing the plate row by row in front of the microscope; the count is made mechanically by a "billiard-marker," and recorded with a pencil at the end of the row. The eye is not removed from the instrument until the end of the row. I mention that as it accounts for the fact that sometimes stars are missed in this counting, which is not extremely accurate, although it is accurate enough for our purpose. The point having arisen as to the relative numbers of stars on the outside edges of the plate and in the centre, in the first instance one of the computers was directed to put down for a number of plates—100 plates—how many stars there were in the two outside rows, and how many in the central row. I thought at first that would be the general nature of the phenomenon, that in the centre of the plates there would be most stars, tailing off gradually to the edges. I shall have occasion presently to mention that this is not generally the case. We had five groups of 100 plates each—the outside rows contained 74, 77, 79, 72, and 94 stars, and the rows next showed 82, 86, 86, 86, 98, taking the central rows as 100 stars each; and I may as well at once draw attention to the fact that group 5 was anomalous. The counts were later in order of time, and as a matter of fact they refer to plates which were taken after the dismounting of the instrument. We had to build a new dome at Oxford recently, and the instrument was taken to pieces. The remounting apparently produced a considerable difference in the percentage in the number of stars in the outside row compared with the centre. After this preliminary investigation it became obvious that the matter was worthy of some attention. We took 180 plates in zone +25°, a whole zone in the heavens, and formed the total number of stars *measured* in each one of the 26 × 26 réseau squares. After the count it was seen clearly that the systematic errors had not been entirely eliminated, these being due to the distribution of the stars in the sky. What we are trying to get at is the optical effect of the object-glasses independently of the distribution of the stars in the sky; and there was a greater number of the stars in the north of the plate than in the south, as is easily seen on referring to the diagram which Mr. Bellamy made of the distribution of stars in our belt of the heavens a year ago. To eliminate this error the mean was taken of two pair of squares situated symmetrically in respect of the centre. When that was done, it was further found that there was no particular advantage in counting the whole plate; the diagonals were sufficient. A detailed analysis was carried out with the Oxford plates, and it was found enough to take the two diagonals; therefore when observations were taken with other plates, the two diagonals were counted simply. The other plates were the beautiful charts published by the French observatories, the long-exposure plates published by the observatories of Paris, Algiers, Bordeaux, and Toulouse, and now by San Fernando. From Paris we have counted 30, Algiers 20, Toulouse 40, San Fernando 10. When all these plates were counted, it was found that the maximum number of stars was by no means always in the centre. In fact it was never in the centre. So far from the centre being the richest region, the richest region was in a ring round the centre, and the radius of this ring may be taken roughly as for Oxford 37′, for Paris as much as 60′, Algiers 49′, Toulouse 39′, San Fernando 35′. At first, I naturally thought these differences between the various observatories were due to differences in the performances of the object-glasses at the different observatories; but a little discussion of the results put one on the track of what I believe to be the real reason of discordance, namely that, in trying to get the best compromise in taking a photograph, *the plate is put a little within the focus.* The best focus for a single object-glass is on a curved surface, and a flat plate crosses it in the circle which gives the maximum density of stars. At any rate if we make that supposition, and assume that the plate is put at different distances in different observatories, we can group all the results in one simple formula with only one disposable constant, which is a sort of thing which gives confidence in having found the true reason. The way in which they fall in with the law is best shown diagrammatically. (Screen.) These diagrams give a comparative statement of the performances of these different object-glasses, or rather, I should say, of the plate when put at a different distance within focus of the same object-glass. I have not described in detail the law which I have assumed for the drawing of these curves, because it is not based upon any precise physical considerations, and I am quite prepared to find that a better law can be suggested. But it did not seem to me worth while to await the formulation of the precise law, because the empirical one put on the screen was sufficient to group the main facts together; and, further, I find it rather difficult to formulate a precise law without a considerable number of experiments. The chief point in the paper is simply this, in all these object-glasses used for the Astrographic Chart we find a difficulty of the same kind in securing uniformity of the field as regards stellar magnitude. It is got over, apparently, by a compromise in the position of the plate, which is different in the different observatories, but it still leaves this discrepancy of something like 5 per cent. in the different parts of the plate. In striking contrast we have the results obtained by our medallist of this year, Dr. Kapteyn. Instead of trying to cover a plate 2° square, the photographs of the C.P.D. cover a plate 6° square, and with practical uniformity. Evidence on this point is given in the C.P.D. itself; but in order to test the matter in a way more or less comparable with that I have been discussing, I have had counts of stars for every minute of R.A. made, and find they bear out what I have just said. The counts show practically uniform density over this large area of 6° square. This raises again the question, which has presented itself rather formidably several times since we started on the star chart, whether we have got the right instrument for the star-charting. The instrument selected in 1887 for taking the plates has certainly several definite advantages for *measuring* star-images. I think all those working with plates to be measured must be glad that it was selected; but a different question arises when we come to the *representation* of the heavens, not necessarily for the purpose of measurement. The accuracy that can be obtained by the photographic doublet has yet to be determined; but even if it were proved insufficient, it might still be that we should be well advised to use a doublet for making what are called the chart-plates—charting the sky so far as the small faint stars are concerned,—especially when we remember that we can cover a large area on each plate. In view of these comparative results, I am emboldened to ask, have we got the right instrument for taking these long-exposure plates?

After one or two questions referring to details:

The President. If no one else has any remarks to make, I am sure we can formally do what the Meeting has already done, and that is to return our thanks to Prof. Turner for bringing this paper before us.

Dr. Rambaut then read a paper by *Mr. W. H. Robinson.*

Recently at the Radcliffe Observatory, in addition to the eye-estimations of the brightness of Nova Persei, Mr. Robinson, on his own initiative, undertook a series of photographs of the region surrounding the new star. Some of the results of this investigation were brought before the Society at the January meeting, and a description of the methods adopted by him will be found in the January number of the *Monthly Notices.* Two of the brighter stars in the vicinity of the Nova, κ and 36 Persei, have been announced in the *Astronomische Nachrichten,* No. 3720, as New Variables, but Mr. Robinson finds on examining his plates that they lend no support to this view. In the paper before the Society Mr. Robinson reprints Dr. Guthnick's values for the magnitude of κ Persei. In this series the differences of the separate observations from the mean range from $+0^{m}\cdot59$ to $-0^{m}\cdot23$. The results point to a maximum on or near March 21 and a minimum about April 23. Fortunately the star was observed at the Radcliffe Observatory on both of these crucial dates by three observers. Their results, instead of confirming a *fall* of $0^{m}\cdot6$, indicate perhaps a *rise* of $0^{m}\cdot1$, or more probably a *constancy* in brightness. In this series of observations the extreme differences from the mean are $+0^{m}\cdot16$ and $-0^{m}\cdot18$. The *plus* and *minus* signs were fairly evenly distributed, as one would expect if the discrepancies were due to errors of observation. The photographic results are even more accordant. Mr. Robinson made three different series of estimates of the densities of the star-trails on the plates, and from the means of the three he obtained the magnitude, the extreme differences in this case being $+0^{m}\cdot12$ and $-0^{m}\cdot11$.

Similar results were found in the case of 36 Persei, the extreme differences in this case being $+0^{m}\cdot13$ and $-0^{m}\cdot11$. In each case Mr. Robinson found that the observations were best represented by a horizontal straight line, and he concluded that the evidence afforded by the photographs and the Radcliffe eye-estimations seemed to give an emphatic negative to the hypothesis of the variability of these two stars.

Since this paper was written a note had appeared in the *Astronomische Nachrichten*, No. 3779, in which the observers at Potsdam stated that in the interval between 1900 Oct. 9 and 1902 Feb. 15 they had observed 36 Persei 15 times and κ Persei 22 times between 1898 March 13 and 1902 Feb. 15; that in the case of both stars they had found a remarkable constancy in brightness; and that much more conclusive evidence than had hitherto been produced must be forthcoming in order to convince them of the variability of these stars.

Mr. Bryant. Were the Oxford observations taken at the same altitude? and was there any allowance made for differential atmospheric absorption?

Dr. Rambaut. At the time the photographs were taken the stars were fairly well up at Oxford. The comparison stars selected by Mr. Robinson were very close to the stars under observation, so that the correction for atmospheric absorption would be negligible.

Mr. Bryant. I meant differential absorption depending on colour.

Dr. Rambaut. The colour would doubtless have had an effect, though small, upon the results.

Mr. Hinks. Does Mr. Robinson measure the breadth of the trail or its density? How does he use the scale? It seems to me it is an interesting point. The only information which has been published is in some of the Harvard volumes, and you can get the magnitude very accurately indeed by comparing the scale of intensity.

Dr. Rambaut. I think this is a very important point which Mr. Hinks has raised. In the case of Mr. Robinson's photographs the estimates of density were made differentially. He compared the density of the trails of 36 and κ Persei with those of other stars of the Harvard Photometry and certainly the results agree in what appears to be a very striking manner. In this paper Mr. Robinson has given no further particulars as to his method, but in his previous paper he has described more or less completely how the trails were measured.

The President. I am sure the Society will thank Mr. Robinson for his paper, and Dr. Rambaut for his exposition of it.

Mr. Wesley then showed on a screen a photograph of the trail of a meteor, and said that, so far as he recollected, it was the sixth meteor photograph which had been taken. The first was when Prof. Barnard accidentally got the trail of a meteor on a plate which he was exposing for comet Brooks in 1893; and in 1895 Mr. Butler of South Kensington secured a beautiful one, and in 1897 Prof. Barnard obtained two, and another in 1899. In this case Prof. Rees and his friend were exposing with a view of getting pictures of the Leonids. They hoped to get in the summer trails of other meteors and they would certainly try to obtain photographs of the Leonids in November next *.

Mr. Whittaker then read a paper by *Mr. C. T. Whitmell* on "Saturn seen through the Cassini division." He said that the paper related to an interesting occurrence which might be expected in July next, when the planet Saturn would be in opposition. The ring would be projected on the surface of the planet in such a way that part of the planet seen behind the ring would not be in shadow. At the opposition in July next the dark rift in the ring would be over part of the planet. When that happened in general the part of the planet seen behind the rift was in shadow, but on this particular occasion it would not be so, and consequently they would see this rift bright—not quite so bright as the rest of the ring. There would also be the additional interesting fact that a transit of the Earth across the Sun could be seen from Saturn on this occasion.

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* I omitted to mention a photograph showing a meteor-trail in Perseus, taken by M. Antoniadi in July 1897, of which the Society has copies. A copy of a photograph by Mrs. Maunder, showing a trail (the original plate shows two), was published in *Knowledge*, April 1899. Prof. Herschel has also kindly reminded me of the following meteor-photographs: Jan. 1893, long trail, photographed by J. E. Lewis at Ansonia, U.S.; Nov. 14–15, 1898, sixteen trails photographed by Dr. Elkin at Yale Observatory and a neighbouring station; Nov. 1898, meteor-photographs were taken by Prof. Pickering and his coadjutors at Harvard and Blue Hill Observatories; July, Aug., Nov., and Dec. 1899, five trails photographed at Yale Observatory and a neighbouring station by Dr. Elkin and his coadjutors.—W. H. W.

Mr. Dyson then read a paper by *Prof. Barnard* on the probable motion of some of the small stars in the Dumb-bell nebula.

Dr. Rambaut. I should like to say how pleased I am to find that Prof. Barnard has been working on this subject. I was aware that some years ago he had been making measures of these stars. In the present communication he refers to a paper by Mr. Wilson and myself, published in 1894 in the *Monthly Notices.* In this paper we gave the measures of several stars, and compared them with the observations made by Otto Struve. At that time we had no other results to compare them with except some measures made at Lord Rosse's Observatory by Dr Copeland, the present Astronomer Royal for Scotland*. The Parsonstown measures were hardly accurate enough to be relied upon strictly, on account of the great difficulty there was in determining the position-angle with the great telescope at Parsonstown, but they seemed to indicate that the differences between our results and those of Struve were real. Probably, as Prof. Barnard concludes, they must be attributed to errors in the positions as determined by Struve. [Dr. Rambaut here wrote upon the blackboard some figures showing how the Parsonstown results corroborated those obtained by Mr. Wilson and himself.] I think we ought to express our thanks to Prof. Barnard for his valuable paper, which undoubtedly shows that these differences must be attributed, not to the proper motion of the stars, but, mainly at all events, to errors in the earlier measures.

Mr. Dyson then read a paper by *Mr. R. T. Innes*, on the Cape Double-Star results, 1901. The writer said that the weather was unfavourable for double-star work, but during the year several hundred observations were made.

The following papers were announced and partly read:—

Royal Observatory, Cape of Good Hope. "Cape Double-Star Results, 1901."

Royal Observatory, Cape of Good Hope. "Notes on Nebulæ."

W. S. Aldis. "Explanation of use of Tables of $\frac{1}{2}(\theta + \cos\theta)$."

H. H. Turner. "On Stationary Meteor-Radiants." Third Paper.

J. Tebbutt. "Results of Double-Star Measures at Windsor, New South Wales, in 1901."

C. T. Whitmell. "Saturn seen through the Cassini Division."

E. E. Barnard. "On the probable Motion of some of the small Stars in the Dumb-bell Nebula."

W. H. Robinson. "On the supposed Variability of κ Persei and 36 Persei, and a comparison of the photographic and visual magnitudes of those Stars."

H. H. Turner. "On the relative number of Star-images photographed in different parts of a plate, and on the performance of various Object-glasses in this respect."

---

* Dr. Copeland's name was quoted in error for that of Mr. Bindon Stoney.

The following gentlemen were elected Fellows of the Society:—

G. H. Bryant, Higher Grade School, Cinderford, Gloucestershire.

Capt. G. P. Lenox-Conyngham, R.E., Survey of India, Mussoorie, India.

S. A. Mitchell, A.M., Ph.D., Columbia University, New York City, U.S.A.

Jas. Rankine, 11 West Saville Road, Edinburgh.

Thos. Thorp, Moss Bank, Whitefield, near Manchester.

W. B. Whall, H.M. Board of Trade, Liverpool.

The following Candidate was proposed for election as a Fellow of the Society:—

H. B. Adames, 8 West Street, Chichester (proposed by W. W. Bryant).

THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1902 March 26.

Mr. G. M. Seabroke, F.R.A.S., *President*, in the Chair.

Secretaries: A. C. D. Crommelin, F.R.A.S., and
J. G. Petrie, F.R.A.S.

The Minutes of the previous Meeting were read and confirmed.

Mr. J. G. Petrie read the list of presents received.

The names of seven Candidates for admission were also read and passed for suspension, and the election of four new Members by the Council was confirmed.

Mr. Henry Corder read a paper on "The Sun-Pillar of March 6th." The pillar was first seen before sunset, about 5 P.M., when it was white in colour and not many degrees high. At sunset it was very much higher and had become amber-coloured; but its chief grandeur took place some little time afterwards, when it gradually increased in intensity of colour to a rich orange-scarlet. There could be no doubt that this was a smoke effect. In that district, whenever the wind was in the quarter from E. to N. the air, after about 24 hours, became smoke-laden, either from London or the central districts of England. On some few occasions he had seen fine sun-pillars of the lovely rosy hue proper to sunset phenomena, but on this date the air had been smoky for several days and the sun had set red. This colour therefore was imparted to the pillar, which was no doubt formed by reflection from the underside of a cloud of ice-crystals, as the pathway of light was formed on the sea by Sun or Moon. As the sky darkened, the pillar seemed to glow like fire. It finally died down about 6.40, appearing to become withdrawn into itself. The width of the pillar at the bottom would be about that of the Sun, and not very much greater at the top—perhaps one degree.

The Rev. G. J. Bridges also had a paper on the same subject. He said the column of light appeared almost, if not quite, suddenly, and could at its highest period be just traced to the zenith, was of even width and perfectly perpendicular, moved with the Sun when above and below the horizon, and took the local colour of the farthest tints of the sunset, but in more intense and lasting form. These colours were intensified after sunset. A distinct pulsation could be traced, chiefly at the apex.

Miss Catherine O. Stevens said that shortly after 6 o'clock on the evening of March 6 she observed that from the point upon the horizon where the Sun had disappeared rose a glorious parallel-sided shaft of brilliant rose-red light, which traced a length of some 20° or more towards the zenith. She judged the width to be between 1° and 2° and its form parallel-sided from base to summit. It sank out of sight below the horizon at 6.28 P.M. She came to the conclusion that, whatever the phenomenon might be, it could not be a creation of the Earth's atmosphere, excepting only in respect of its colour, and that it was not of the nature of a halo of any sort, but a true solar appendage. From friends she learned that as early as 5.30 P.M. the shaft was already conspicuous, but was at that time a sun-coloured object, and springing from the top of the Sun, and that it "flushed" as it sank into the denser strata of our atmosphere.

The Rev. Septimus Herbert spoke to having seen the sun-pillar alluded to in the paper just read. He first observed it at 6.15 P.M. at Harrow, and watched it till it faded away at 6.30. It was of a bright rosy tint. It appeared to be about 1° in breadth and from 8° to 10° in vertical height and shortened in length before it faded away.

The President observed that a pillar of the sort described, but inverted, was seen when one looked at the reflection of the Sun when near the horizon in rippled water. A shaft of light was then apparent, proceeding downwards from the Sun. In order to give the appearance of the pillar described with parallel sides, one must imagine a rippled or rough inverted reflecting surface very approximately plane. It was somewhat difficult to account for such a surface; but unless that was the case, one would not expect to find the shaft's sides parallel. It was right for an observer to look near home first, and he would scarcely go to the Sun, or to the near proximity to the Sun, for the source of this sun-pillar, unless every suggestion to account for it from some atmospheric cause had been exhausted. The observations related that night were difficult to account for from any terrestrial cause, but no doubt there was some such cause for them.

Mr. Walter Maunder asked the lanternist to throw on the screen a slide of a drawing, made by Miss Stevens, of the recent great sun-spot. This was followed by a slide, made by Mrs. Maunder, of the sketch of the sun-pillar of February 30, 1895, drawn by the Rev. S. Barber, and published in *Knowledge* for June 1895. The Rev. S. Barber took the same view of the origin of the appearance as the President and Dr. Johnstone Stoney, namely—that it was caused by the reflection of sunlight from the under surface of particles of ice floating horizontally in the air. As the President had said, it was a very similar effect to the track of light proceeding from the Sun seen on the surface of water, and which was represented as well as the sun-pillar in Mr. Barber's picture. Mr. Maunder then showed a third slide, which he had drawn in order to show the position of the ecliptic at sunset on March 6, and the mode in which the pillar, if a solar appendage, should move in altitude and azimuth as the Sun went down. He said he had been particularly interested to find that the Rev. Guy Bridges alone amongst the observers of the pillar had drawn attention to the movement of the pillar in azimuth.

Mr. A. C. D. Crommelin remarked that the apparent motion of the shaft would have been the same whether it was an atmospheric effect or a truly solar appendage. He thought if it had been a true solar appendage it would have remained longer than Miss Stevens saw it; in fact it seemed to have behaved very much in the way one would have expected if it had been in the upper regions of the atmosphere.

Mr. Maunder then showed a number of photographs of the great sun-spot group of May 1901, and of the groups of January and March 1902. He also showed some photographs of the eclipse of May 18, 1901, disclosing a great disturbance in the corona immediately above the place of the spot group.

Capt. William Noble contributed a paper on the "Signs of the Zodiac." He said that, interesting as was the origin of the Signs of the Zodiac from a historical and archæological point of view, he ventured to think that their retention now in practical astronomy was a quaint anachronism and mistake, unjustifiable alike on scientific grounds or on those of use or convenience. In fact, the temptation to the student to confuse and confound the zodiacal signs with the constellations whence they originally derived their names was very great indeed, and had led many a beginner into a trap. It was stated in the *Nautical Almanac* for the current year that on March 21st, at 1^h, the Sun entered Aries. In point of fact the Sun was at that time more than 28¼° from the very nearest part of that constellation. There was no practical convenience in the present preposterously artificial arrangement. He would substitute for the present signs, longitude 0°, 90°, 180°, 270°. He would say: March 21 1^h, Sun's longitude 0°. Spring commences, &c. Surely nothing but superstition caused them to adhere to those old astrological symbols.

Dr. Downing related a somewhat amusing experience in connection with the subject under discussion, and added that in order to meet the difficulty he had altered the phraseology to "the Sun enters the Sign Aries."

Mr. Crommelin thought there was much to be said in favour of Capt. Noble's suggestion, but he should deplore the disappearance of the names as attached to the constellations themselves; the latter were full of interest from an archæological and sentimental point of view.

Mr. J. H. Bridger dealt with "The Visibility of Meteors from Radiants near the Horizon." The principal conclusions at which he arrived were:—(1) It was possible to see a meteor at a low altitude in the direction of its radiant about half an hour before the radiant rose or after it had set. Such a meteor would be at a great distance from the observer (not less than 350 miles) and have a flight approximately parallel to the Earth's surface below it. (2) When a radiant was on the horizon (rising or setting) it might supply a meteor in the zenith, which would be at a considerable height, with flight nearly horizontal to the Earth's surface. A meteor in the opposite part of the sky was not possible, as the length of atmosphere it would have to traverse would destroy it before reaching the position. (3) Conversely, the radiant of a distant meteor of a low altitude could not be more than 10° below the horizon, and the radiant of a meteor in the zenith must be on or above the horizon. (4) About 45^m after a radiant had risen it might supply meteors in any part of the sky.

Mr. J. Gavin Burns read a paper on "The Distribution of Double Stars in Space." The writer concluded that theoretically two facts would be expected to appear on the assumption of an even distribution. First, that if double stars were classified according to the distance of their components, the closer pairs should always greatly exceed the wider pairs in number. Second, that if double stars were classified according to their magnitude, the stars of fainter magnitude should always be in excess. An analysis of the stars contained in Struve's 'Mensuræ Micrometricæ' for the northern hemisphere and Innes' Catalogue for the southern did not give the anticipated result. There were only two ways of accounting for this. It might be owing to the fainter stars generally being much nearer than was implied by their magnitude, or it might be owing to the comparative proximity of double stars.

Mr. Crommelin said there was no doubt that where they knew both the apparent distance and period of a double star, a good idea at all events could be formed of the order of distance to which it belonged, but they should at least know for certain whether the star was really a physical double or only an optical one.

The President doubted whether enough stars were known to be certain binaries to make a sound foundation to work upon at the present time. There were a great many stars that showed motion, but whether it was due to their being binary systems was quite another matter. He thought that investigation in this direction should be confined to known binaries only.

The Meeting was then adjourned to April 30.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society held on Wednesday evening, the 16th instant, at the Institution of Civil Engineers, *Mr. W. H. Dines*, the President, in the Chair,

Capt. D. Wilson-Barker delivered a lecture on "Clouds." After some remarks on the composition and the height of the atmosphere, the lecturer said that until recent years comparatively little scientific attention had been paid to the subject of clouds. This he largely attributed to the lack of a simple practical classification. The French naturalist Lamarck was probably the first to formulate one, but Luke Howard, a London merchant, about 1802 introduced the first practical classification, which is still in use among many observers. Clouds are formed by one of two causes, viz. (1) the mixing of two masses of moist air of unequal temperatures, or (2) through changes occurring in the atmosphere where expansion and consequent loss of heat take place, causing condensation of moisture. Capt. Wilson-Barker said that a simple primary classification is best arrived at by a two-fold division of cloud types, viz. (1) "stratus" or sheet clouds, and (2) "cumulus" or heap clouds. The former may be roughly considered the cloud of a settled, and the latter of an unsettled state of the atmosphere. He showed by means of lantern-slides a number of cloud-pictures illustrating certain varieties of both main types. Under "stratus" or sheet cloud the lecturer included fog, stratus, high stratus, cirro-cumulus, cirrus, nimbus, and scud; and under "cumulus" or heap clouds he included the ordinary cumulus, the shower cumulus, the squall cumulus, and roll cumulus.

In conclusion, Capt. Wilson-Barker referred to various optical phenomena associated with clouds, such as coronæ, halos, sun-pillars, rainbows, and also the colour of the sky.

Réglement du Prix institué à l'Académie Impériale des Sciences de St.-Pétersbourg par l'Académicien Th. Brédikhine, en Souvenir de feu son Epouse, Anna Brédikhine.

(1) Avec les intérêts du capital de *six mille vingt* roubles (6020 roubles, env. 16000 francs) déposé à perpétuité par l'académicien Th. Brédikhine à la Banque de l'Etat, en souvenir de son épouse Anna Brédikhine (née Bologovskoi), il est institué un Prix d'encouragement à la continuation des travaux de l'académicien Th. Brédikhine sur les recherches du développement des formes cométaires à un point de vue rigoureusement mécanique.

(2) Le Prix doit être immuablement nommé "Prix d'Astronomie ANNA BRÉDIKHINE, épouse de l'académicien."

(3) Le Prix provient des revenus bisannuels (env. 1000 francs) du capital déposé; il en sera déduit la valeure d'une médaille d'argent destinée au juge. Le prix est décerné pour les recherches sur une grande comète. Ces recherches doivent être appuyées d'un nombre suffisant d'observations obtenues de préférence à l'aide de la photographie.

(4) Les recherches mécaniques du développement des formes d'une pareille comète doivent être faites d'après le même plan et avec les mêmes formules exactes du mouvement des particules cométaires qu'appliquait l'académicien TH. BRÉDIKHINE dans ses travaux sur les comètes (voir les Annales de l'Observatoire de Moscou 1875–1900 et les Bulletins de l'Académie Impériale des Sciences à partir de 1890). Les transformations et les changements des formules introduits dans le but de faciliter les calculs, ne peuvent être un obstacle à l'obtention du Prix.

(5) Sont soumises à des recherches les grandes comètes qui paraitront à partir de 1902.

(6) La présentation des travaux au concours du Prix est fixée au 10 septembre (jour de décès d'ANNA BRÉDIKHINE)—pour les années impaires, et la date d'adjudication du Prix est fixée au 10 septembre des années paires.

(7) L'ouvrage présenté peut avoir (1) ou la forme de brochure séparée, imprimée dans une des langues les plus connues des astronomes de toutes les nations, c'est à dire dans une des langues prédominantes dans la littérature astronomique; (2) ou la forme d'un extrait de publication scientifique dans pareille langue. En pareille langue encore (3) on peut même présenter un manuscrit; mais le Prix ne sera décerné qu'après l'impression de l'ouvrage, et après remise de deux exemplaires au secrétaire perpétuel de l'Académie.

(8) Les Etrangers, tout comme les Russes, sont admis au concours. Dans ce but, un an avant le terme de la présentation des travaux, l'Académie annonce le Prix et ses conditions dans un des journaux les plus répandus parmi les astronomes de toutes les nations (par exemple les Astronomische Nachrichten etc.). Quant à la critique des travaux, l'Académie peut la confier aussi bien à des astronomes étrangers reconnus compétents qu'aux astronomes russes.

(9) Au cas où des prix bisannuels, pour une cause ou une autre, n'auraient pas été décernés aux termes assignés, les sommes en seront mises en réserve, et quand le nombre de prix non adjugés s'élévera à cinq (c. à d. env. à 5000 francs), le total de ces cinq prix mis en réserve, y compris les intérêts, et déduction faite d'une certaine somme nécessaire à l'acquisition d'une médaille d'or pour le juge, sera adjugé sous forme de "grand Prix" à un Traité suffisamment complet sur tous les travaux de l'académicien TH. BRÉDIKHINE relatifs du développement des formes cométaires, ainsi que sur des ouvrages déjà primés conformément aux régles

précédentes. Un pareil traité doit contenir la comparaison de tous les résultats numériques ainsi que toutes les généralisations et explications possibles dans l'état des sciences physiques à l'époque correspondante.

(10) Le "grand Prix" sera de même nommé "Prix d'Astronomie Anna Brédikhine, épouse de l'académicien." Les conditions de langue et de nationalité des auteurs sont les mêmes que pour les prix ordinaires, ou, autrement dit, décernés bisannuellement.

(11) En cas de formations subséquentes de pareils grands Prix, l'Académie peut, si elle le trouve opportun par suite d'une accumulation suffisante de matériaux, annoncer chaque fois un concours dans le but de compléter et de corriger le Traité mentionné, d'après les travaux primés ultérieurement. Dans le cas contraire, l'Académie peu ou augmenter pour un certain nombre d'années la somme des petits prix, ou, par concours, décerner les grands prix à des travaux sur différents problèmes intéressants de l'Astronomie Cométaire en général, ou sur la théorie des courants météoriques. Dans tous ces cas, le Prix garde son nom et conserve les conditions susmentionnées en ce qui concerne et la langue et la nationalité des auteurs.

(12) Après la formation d'un capital de cinq petits prix non décernés, l'Académie porte, comme il a été dit, à la connaissance générale des astronomes l'ouverture du concours du 10 septembre, en posant pour condition que les travaux sur le sujet imposé soient présentés dans le délai de deux ans, le 10 septembre, et que l'adjudication du Prix soit annoncée le 10 septembre de l'année qui suivra la présentation.

(13) Sur chaque édition couronnée du grand Prix il doit être imprimé qu'elle est honorée du "Prix Anna Brédikhine, épouse de l'académicien."

Remarque. La première présentation des ouvrages pour le Prix aura lieu en 1903, et il sera décerné pour la première fois en 1904.

Markings in the North-temperate Region of Jupiter.

In a paper published in the *Monthly Notices R. A. S.* for 1898 December (vol. lix. pp. 76–9), I referred to a possibly recurrent disturbance, taking place at intervals of about ten years, in the north-temperate belt of Jupiter, and pointed out that a repetition of the event might be looked for in 1901. The particular disturbance alluded to consists in the outbreak of a number of dark spots with motions of great velocity, and exhibiting rotation-periods even shorter than those afforded by the equatorial markings.

I made frequent observations of Jupiter in 1901, with a view to redetect the special objects described, but failed in distinguishing them. The north-temperate region of the planet appeared in a

greatly disturbed condition, it is true, and the duple belt lying there was strongly knotted with dark spots. But definition was usually so bad (the planet's S. Dec. being 23½°) that the details were confused, and difficulties arose in following the spots from night to night and identifying them satisfactorily. On September 15, 25, October 19, and several other evenings, however, the seeing was pretty good, and transits were secured of the principal markings in this region. A comparison of these soon proved that the objects were travelling at rates not differing materially from that of the Red Spot, and that, therefore, they could not represent a return of the particular objects looked for. Several of the spots were traced until October 31, and it was found on sorting out the observations, that the mean period of rotation of six of the more conspicuous markings was

$$9^{h}\ 55^{m}\ 50^{s}{\cdot}2.$$

This practically coincides with the mean of two spots seen at Bristol in 1898, between 30° and 40° N. lat. (*Monthly Notices*, lviii. p. 484), which gave

$$9^{h}\ 55^{m}\ 50^{s}{\cdot}15,$$

and with another marking observed here in 1899 (*Monthly Notices*, lix. p. 572), for which the period was

$$9^{h}\ 55^{m}\ 53^{s}{\cdot}5.$$

In 1899 there was another spot in approximately the same latitude, which moved with much greater celerity, for while on February 2 it followed the Red Spot by 30 minutes (=18°), it preceded that object at the end of July by 56 minutes (=34°), and its mean period was

$$9^{h}\ 55^{m}\ 29^{s}{\cdot}8,$$

so that its rate was consistent with that generally shown by the markings situated in the N. tropical current of Jupiter.

For comparison I subjoin a few other recent determinations of the rotation periods of spots in the N. temperate region :—

Year.	Object.	Lat.	Rotation.	Observer.
		°	h m s	
1896.	Dark spot	+26	9 56 0·4	G. W. Hough.
1897.	„	+25	9 55 59·7	„
1898.	„	+24	9 55 52·0	„
„	Dark streak ...	+33	9 55 50·7	T. E. R. Phillips.
„	„ ...	„	9 55 52·0	„
1899.	Dark streaks ...	+25	9 56 6·4 *	P. B. Molesworth.
„	„ ...	+39	9 56 17·5 †	„
„	Dark spot	+24 to +33	9 55 54·2	T. E. R. Phillips.
„	„	„	9 55 54·9	„
„	White spot ...	„	9 56 1·5	„
1901.	Dark spot	+40	9 55 56·0	J. Comas Solá.

* Mean of 6 spots. † Mean of 5 spots.

In 1880 Prof. Hough found a mean period of $9^h\ 55^m\ 35^s{\cdot}1$ for three spots in Lat. $+34°$ to $+37°$. Possibly the motion of this current has slackened during the last 20 years, like that of the rotation period of objects near the equator, which in 1881 was $9^h\ 50^m\ 7^s$, while in 1901 it was $9^h\ 50^m\ 29^s$.

The mean of 20 of the more nearly conformable periods furnished by N. temp. spots is

$$9^h\ 55^m\ 53^s{\cdot}8,$$

and this rate probably represents the principal drift in the region named, but it must be admitted that the latitudes of many of the markings observed were not accurately determined. It seems desirable that more attention should be given in future years to fixing the exact positions both of the belts and spots, for different velocities are characteristic of different parallels, though the rate is liable to certain variations. In Jupiter's south-temperate region there are two well-defined currents with periods of $9^h\ 55^m\ 6^s$ and $9^h\ 55^m\ 19^s$ respectively, and these appear to have remained invariable during a long series of years. The Red Spot is situated in the latter stream, though not running level with other objects in it, for its present rate of $9^h\ 55^m\ 41^s$ causes it to lose 22 seconds in one rotation, or 53 seconds in a terrestrial day.

But in the north-temperate region of the planet a different state of things apparently prevails. "Here," as Mr. A. Stanley Williams has stated, "in close juxtaposition have been found both the quickest and the slowest of the great Jovian atmospheric currents." While in 1880 the motion of spots in a belt in N. lat. 25° indicated a period of $9^h\ 48^m$ only, the same region and that somewhat further north have in recent years shown markings with a rate of $9^h\ 55^m\ 53^s{\cdot}8$. On the whole there seems little doubt that the north-temperate dusky belts and light zones form the seat of greater activity and variation than the south-temperate region.

With regard to the rapidly moving spots which diversified the north-temperate belt in 1880 and 1891, their non-appearance in 1901 does not finally negative the idea of their periodical recurrence. They should be closely looked for during the present year, for, in the event of their return, they will well repay study in combination with other features in the same region. The conditions are now more favourable than at the last opposition, for the altitude of the planet is 5 degrees higher, and this should effect a great improvement in the character of the definition.

Bishopston, Bristol,
1902, April 22.

W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Ancient Eclipses.

GENTLEMEN,—

Mr. Lynn mistakes the import of my letter on the above subject. I did not express a doubt as to whether there was a war between the Medes and the Lydians and also a war between the Medes and the Persians, nor did I suggest that both wars had been carried on simultaneously. What I doubted was whether both wars had been put an end to by the incident of a total eclipse of the Sun, by which the Medes were terrified. If the incident only occurred once, either Herodotus or Xenophon has made a mistake as to the occasion on which it occurred. Neither eclipse (if there were two) is, I apprehend, of the slightest value as regards checking or improving our lunar tables. What is the earliest eclipse that *can* be used for the purpose?

There is one eclipse, however, to which I wish to call attention, that recorded by the Roman historian Ammianus Marcellinus, which Mr. Johnson sets down for Aug. 28, A.D. 360. This date is probably correct. It is hardly possible to doubt that the year at least is so. It was seen by the Roman army engaged in war against the Persians. The historian was with the army and gives a somewhat lengthy and detailed account of a partial eclipse of the Sun. Without directly stating that he was an eye-witness, it is hardly possible to doubt that he was so. But Mr. Johnson states that this eclipse was visible only to the east of the Persian Gulf, whereas the Roman army was to the west of it all through the campaign. This fact seems to me to prove that the tables used in the computation were inaccurate. The eclipse was visible to the west of the Persian Gulf, because Ammianus and his companions saw it there. I remain, truly yours,

W. H. S. MONCK.

Rays of Light from Sun.

GENTLEMEN,—

Gen. Tennant's letter in the *Observatory* for this month recalls to me that I observed one or two brushes of light proceeding from the crescent on 22 December, 1870, when nearly five-sixths of the solar disc were obscured. My place of observation was near Exeter, where the magnitude of the eclipse was given in Francis Moore's Almanack as 9 digits 54′. The impression given at the time was that the cause was a slight bedewing of the object-glass; but such was not the case. The cause could scarcely have been the reflection of sunlight on icy particles in the air, though the thermometer stood at 25° in the shade at the time, as, in this case, they would probably have been more widely noticed.

This may be the place to mention some other rays of light spoken of in a note on p. 477 of Dick's 'Practical Astronomer.' He says that M. Serres, Sub-Prefect of Embrun, on the occasion of the well-known eclipse of 7 September, 1820, saw a large group of individuals gazing upwards, and on questioning an intelligent artist among them, was told, "We are looking at the stars which are detaching themselves from the Sun." He saw an appearance like fire projected from the upper hemisphere of the Sun. This gradually ceased as the eclipse wore off. One of the crowd remarked that "the Sun projected most stars at the time when it was palest." [*Query*, Was this a meteorological phenomenon, or some daylight meteoric display?] Seeds in the air would scarcely be sufficient explanation.

Melplash Vicarage, Bridport.
1902, April 7.

Faithfully yours,
S. J. Johnson.

Alleged Eclipse at the Death of Queen Anne.

Gentlemen,—

I have recently met with an instance of how an occurrence is sometimes invented to account for a misunderstood expression, which may perhaps amuse some of your readers. In a letter from Swift to Pope, which is given in Sir Walter Scott's Works of Swift (2nd edition, vol. xvi. p. 228), and is dated Dublin, June 28, 1715, the Dean writes:—

"Upon the whole you may truly attribute my silence to the eclipse, but it was that eclipse which happened on the first of August." On this a note by Bowles is quoted: "There was a great eclipse at this time. He alludes to the death of Queen Anne on the 1st of August." And Mr. Sichel, in his recent work 'Bolingbroke and his Times,' improves upon this by stating (p. 497), after mentioning the Queen's death, "It was the day, as the superstitious noted, of an eclipse of the sun."

As a matter of fact the Moon was new two days before, on July 30 (O.S.), 1714, and there was no eclipse even partially visible in Europe that year. Swift therefore was using the expression eclipse in a metaphorical sense for the fact of the death of the Queen; but the first clause of the sentence probably alludes to the real total eclipse of the Sun which occurred on April 22 (O.S.) in the following year. And so it is taken by Mr. Elwin in a note on the same letter given in his edition (vol. vii. p. 10) of the 'Works of Pope.' The metaphorical sense of the word "eclipse" in the second clause reminds me of an anecdote of the late Prof. Pritchard, told me by Main when Radcliffe Observer. The former had an illness soon after removing to Oxford as Savilian Professor, and on Main going to see him, his first remark was: "Oh, here's an eclipse!"

Yours faithfully,
W. T. Lynn.

Blackheath, 1902, Apr. 2.

Comet referred to by Milton.

GENTLEMEN,—

The owner of the Oxford Note-Book asks, in your April number, for suggestions respecting a well-known passage in the second book of 'Paradise Lost,' as to whether this refers to the appearance of an actual comet, and if so, which. Now Gibbon, in a note at the end of his forty-third chapter, threw out the very probable conjecture that it refers to the comet of 1664. It is true that before that time Milton had become totally blind, so that he could never have seen the comet; but he must have heard its appearance discussed. It is also true that the great Miltonian authority, Prof. Masson, thinks that the first two books of 'Paradise Lost' were composed before the Restoration, *i. e.* before 1660; but it is quite possible that alterations and additions may have been made in these before the poem (which was not published until 1667) was completed in 1665. The latter date was the year following the appearance of the comet in 1664, the year of the declaration of war with Holland and of the first outbreak of the terrible plague which carried off so many thousands in London in 1665, a coincidence which may well have suggested the lines:—

> From his horrid hair
> Shakes pestilence and war.

As to its being said of the comet, in the preceding lines,

> That fires the length of Ophiuchus huge
> In th' arctic sky,

surely this only shows that the poet's knowledge of the constellations was rather "mixed," for no part of Ophiuchus can be justly so described, and, indeed, its larger part is in the southern hemisphere. Nor does Prof. Masson's note on the passage help the reader in this respect, calling Ophiuchus "a large constellation in the northern heavens."

There had been no other conspicuous comet since 1618, when Milton was only ten years old. We are told that many thought that comet portended the death of Queen Anne (commonly called Anne of Denmark, consort of James I.), though the comet ceased to be visible in December, and the Queen did not die till the following March (1619). Arthur Wilson seems to have prided himself on his superior discernment in considering that what the comet did portend was the war then breaking out in Germany, which in after times was called the Thirty-years' War. That it did indicate some "horrid event here below" he had no doubt whatever; the skill of astrologers was to be evidenced in pointing out *which*. Milton cut the knot in his own time by suggesting that both the pestilence *and* the war were due to the comet; and it seems to me very probable that Gibbon is right in thinking that he refers to the comet of 1664. Yours faithfully,

Blackheath, 1902, April 4. W. T. LYNN.

The Flash-spectrum.

GENTLEMEN,—

In the discussion on Mr. Mitchell's paper on the Flash-spectrum, read at the Royal Astronomical Society on March 14th and reported in your April number, Mr. Fowler mentions a method adopted by me of "attempting to reconcile the two sets of lines (*i. e.* those of the flash and Fraunhofer spectra) by a process of averaging their intensities." This he regards as objectionable on the ground that it suppresses some lines of abnormal intensity in the flash-spectrum to which he attaches a special significance, these lines "in the case of such an element as iron being almost invariably lines which are brighter in the spark than in the arc spectrum."

In my eclipse spectra of 1898 there are 85 lines in the region more refrangible than *h* which have been ascribed to iron; of these 11 may reasonably be considered to be abnormally bright, and nearly an equal number abnormally faint, when compared with their corresponding absorption-lines. The lines of normal intensity, that is those which are in agreement with the dark lines, amount to 73 per cent. of the whole number. In the case of the element titanium, 80 per cent. of the lines are of normal intensity.

It is not therefore a question of "attempting to reconcile" the bright-line and dark-line spectra, which are practically alike for each element, and the differences are largely accounted for when we take into consideration the lines which have a double or multiple origin. For instance, when an iron line falls within a few tenths of a unit of a line of an element relatively strong in the flash by reason of its wide diffusion, such as Sc, Ti, Cr, &c., then the bright line will be unduly strong in relation to the dark line. Of the eleven abnormally strong iron lines mentioned, six can almost certainly be accounted for in this way.

Now as to the supposed relation between the abnormal flash-spectrum lines and the lines which are stronger in the spark than the arc. Only two of the eleven abnormal iron lines of my spectra fall within the limits of the table published by Sir Norman Lockyer* of the enhanced lines of iron, but neither of these are enhanced lines. However, a fair comparison cannot be made with my spectra of 1898, because they do not give good values of wave-length or intensity in the visible region of the spectrum, where the spark-lines have been more particularly studied. But the more recent photographs obtained in 1900 give reliable values for this region, and the results of these, which are in very good agreement with Sir Norman Lockyer's determinations from his Indian spectra and with Professor Frost's results, cannot be said to bear out Mr. Fowler's statements, from which one would infer that the region of the flash is not the seat of the Fraunhofer absorption lines.

Comparing the iron lines with the table of enhanced lines, I find 21 out of 35 are present in the flash; but of these 14 are of normal intensity, leaving 7 only which are unduly strong in the flash. Of these, the most striking examples are the lines at 4584, 4924, and 5018, to which may be added the *ex* corona line at 5317, which is outside the limit of my spectra.

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* P. R. S. vol. lxv.

On the other hand, there are at least four abnormal flash-lines (omitting those which seem accounted for by other elements interfering) which are not enhanced lines. Taking the iron spectrum as a whole, the intensities agree in the main with the dark-line intensities, and, according to my more recent results, some of the exceptional lines coincide with a few of the enhanced spark-lines.

Yours faithfully,

J. EVERSHED.

Kenley, Surrey,
1902, April 15.

GENTLEMEN,—

During the discussion of my paper on the "Flash-spectrum, Sumatra Eclipse, 1901," at the March meeting of the R. A. S., Mr. Fowler remarked, "a discussion of the flash-spectrum which takes no account of the enhanced lines cannot be regarded as final, and I think that if Mr. Mitchell had considered these lines he would have modified the conclusion at which he has arrived." I agree with Mr. Fowler in the first part of his statement; but if the paper read at the meeting had been a *complete* account of my investigations of the flash, instead of a mere abstract, he would have seen that I looked carefully into the question of the "enhanced" lines, as I am aware that Sir Norman Lockyer has laid great stress on the fact that these lines are found with remarkable frequency in the flash-spectrum. By reference to my paper in the *Astrophysical Journal* for March, it will be seen that I too have found a great number of "enhanced" lines, corresponding to flash-lines, for titanium, as many as 30 out of 53 lines of Lockyer's list given in Proc. R. S. lxv. p. 452 (1900); but I also found that "every one of these thirty lines, without exception, appears as a strong line in the solar spectrum"; and the coincidences between "enhanced" and flash lines prove nothing definite, for if the cause of the flash is found in the extent of the vapours—as I take it—where there are strong Fraunhofer lines we expect strong reversals in the flash. A consideration of the remainder of the 53 "enhanced" lines of titanium, and also those of iron, vanadium, and manganese, has led me to the conclusion that "there is no close connection between enhanced lines and the bright lines of the chromosphere seen in the flash."

I therefore consider the difference between the vapours that cause the flash and those that cause the Fraunhofer spectrum is one only of *heights* to which these vapours extend. A low-lying dense layer of great atomic weight might give strong absorption-lines, but as the Moon passes over it so quickly, the emission-lines in the photograph may be very weak; while, on the other hand, a layer of small atomic weight which extends to great distances may be very feeble in intensity and cause almost no absorption, but in view of the long exposure given, as the Moon gradually passes over this layer, the flash-lines in the photograph may be very strong indeed.

It seems to me simpler to explain the flash in this way than to suppose it is due to the higher temperature reached as we go away from the surface of the Sun, which we must assume if we accept Sir Norman Lockyer's explanation.

Columbia University, New York,
1902, April 17.

Very truly yours,
S. A. Mitchell.

PUBLICATIONS.

Micrometer Measures of Double Stars made with the 30-inch Refractor at Poulkova, by Hermann Struve.—These measures, extending over the period 1885–1895, are collected and published as vol. xii., series ii., of the Poulkova Publications. The volume contains measures of 470 Σ pairs, 190 OΣ pairs, 200 Burnham stars, and about 30 miscellaneous systems, making about 900 in all. Double-star observers and others have been looking forward to the publication of these measures with some anxiety and natural impatience. We have been curious to know the capabilities of the 30-inch, and these results have seemed rather long on the road. Now we have them there can be no doubt that they form a valuable acquisition to our double-star measures, more especially those of the Σ pairs, the greater number of which were much needed. And yet we are keenly disappointed. They cannot be representative work of the 30-inch. The pairs measured are out of all proportion to the aperture, and indeed would fit ill in a working list for a refractor half the size. Here is an analysis :—

	Under 0″·5.	0″·5 to 1″.	1″ to 2″.	2″ to 5″.	5″ to 10″.	Over 10″.
Σ	28	43	83	109	49	84
OΣ	38	47	39	20	19	10
β	10	37	42	40	30	18
Sum ..	76	127	164	169	98	112

These do not include second companions, the closer pair only being taken. No doubt sentiment is very much responsible for the Struve pairs, but this fails to account for the Burnham stars. Indeed, when the close pairs are examined there is a feeling that great difficulty was experienced in measuring anything under 0″·4, and yet there are good measures of a few stars, such as :—Σ 2, dist. 0″·34 ; Σ 1639, dist. 0″·2 ; 1728, dist. 0″·2 ; Σ 412 ; δ Equulei ; and ε Hydræ.

For the present, then, we prefer to consider these measures as non-representative and accord them welcome on their merits as observations of pairs much wanted.

STRATONOFF'S CHARTS *.—The second volume of the Publications of the Observatory of Taschkent forms a contribution to the study of the structure of the Sidereal Universe, which seems scarcely to have received the recognition which its high value merits. The problem with which it deals is the vastest in the whole range of physical science, and the difficulties to be overcome in attempting its solution are all but insuperable. Nevertheless there has been a silent but steady progress made during recent years, and the special merit of M. W. Stratonoff's work is that it places before our eyes, in an impressive and readily intelligible form, the results of some of the most recent enquiries.

The material for the determination of stellar distribution handled by M. Stratonoff in the present work is supplied by the Bonn Durchmusterung. This covers the heavens from the North Pole down to 20° of South Declination. For the remainder of the sky the Cape Photographic Durchmusterung is now available, but its discussion will necessarily take much time and M. Stratonoff has therefore preferred to publish the results which he has already obtained rather than hold them back for the considerable interval which must elapse before the work for the southern heavens is as complete as that for the northern.

For the question of the distribution of different types of stellar spectra, the "Draper Catalogue" is the authority. This also confines the enquiry to the heavens north of 20° South Declination. The materials for the enquiry as to the distribution of nebulosities and star-clusters extends over the entire heavens, but are much fuller and more complete for the northern than for the southern hemisphere.

The method adopted for the calculation of the results graphically expressed in M. Stratonoff's charts is essentially the same as that employed by Prof. Schiaparelli in his investigation into the distribution of the lucid stars, published in No. xxxiv. of the Publications of the Royal Observatory of Brera in Milan. The entire sky is divided into 36 zones, each 5° wide, by parallels of declination. These are again subdivided by meridians of right ascension, so that each hemisphere contains 900 areas. The right-ascension lines between $+50°$ and $-50°$ are taken 20^m apart; nearer the pole, a wider interval is employed. Next, for each area, the number of objects per square degree is computed; the number 10 being taken to express the mean density for the northern hemisphere. Further, to eliminate mere accidental effects, the "reduced density" has been taken; that is to say, the density for each area has been obtained by taking the mean of the density of that area and of all the areas immediately surrounding it, usually eight in number. Then in the charts, a light wash of colour is spread over all areas with densities 10 to

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* Publications de l'Observatoire Astronomique et Physique de Tachkent: No. 2. Etudes sur la Structure de l'Univers, par W. Stratonoff, Astrophysicien de l'Observatoire de Tachkent. Première partie. Texte et Atlas. 1900.

15, a deeper wash for densities 15 to 20, and so on: the deepest colour being reserved for all densities over 30. The contour-lines marking the outlines of the regions of greater or less condensation are thus brought out in striking effect.

Lastly, since all these enquiries as to sidereal structure turn on the form and significance of the Galaxy—unrolled as it is, night after night, a constant challenge to astronomers to attempt to extract its secret—the Galactic pole and equator are laid down on the charts, and every tenth degree of Galactic latitude. The position of any particular condensation with reference to the main structure of the Milky Way is therefore seen at once. The coordinates adopted for the north pole of the Galaxy are those used by M. Seeliger, following the determination by Houzeau, viz.:—

$$\alpha = 12^{h}\ 49^{m}{\cdot}1 \quad \delta = +27^{\circ}\ 30' \ (1880{\cdot}0).$$

The charts thus formed are twenty-three in number; the first nine showing the distribution of the stars of Bonn Durchmusterung. Chart 1 deals with the stars down to mag. 6·0 (B.D.) and corresponds therefore very nearly with the chart of the northern hemisphere as given in Planisphere 1 of Schiaparelli's work referred to above. The feature which at once catches attention is the distinct manner in which the axis of greatest condensation deviates from that of the Milky Way; crossing it about R.A. 0^{h}; and trending northwards towards 18^{h}, and southwards towards 6^{h}. Nor is the difference from galactic condensation seen only in the position of the axis. The details of distribution are unlike those of the Milky Way; indeed an area of concentration above the average is found close to the galactic north pole.

It is impossible in a brief notice like the present to refer at length to the characteristics displayed by the eight charts which next follow. Of these Charts 2 to 8 show the arrangement of the stars taken in successive classes of half a magnitude each—class 2 including the stars of B.D. magnitude 6·1 to 6·5, class 3 of magnitude 6·6 to 7·0, and so on; whilst Chart 9 exhibits the distribution of all the stars of mag. 9·0 and brighter. Taking the charts in their order, it is noteworthy that the fainter the class of stars treated, the more nearly does its general distribution approach to that of the Milky Way. Yet the line of maximum never coincides with the galactic equator. In classes 1 and 2 it is inclined to it at an angle of about 20°; in classes 3 and 4 it crosses the galactic equator in Cygnus and Auriga, and bends southward in Cassiopeia, curving strongly northwards in Gemini. This last-named deviation is not shown in class 5, mags. 7·6 to 8·0; and for fainter stars the maximal line after meeting the galactic equator in Auriga curves southward, in the direction of Sagittarius. For no class therefore does it quite coincide with the axis of the galaxy, nor does it resemble any great circle whatsoever of the sphere.

Perhaps the most interesting circumstance which comes out from the detailed examination of this series of charts is the change in several respects which becomes apparent between the lucid stars on the one hand, and the very faintest stars included in the Bonn Durchmusterung on the other. Besides the deviation of the maximal line from the galactic equator, so strikingly seen in classes 1 and 2, the galactic pole is far from being the region of least density; such regions are everywhere near the celestial equator. But when the classes 7 and 8 are reached, these relations no longer hold good; whilst class 6, mag. 8·1 to 8·5, appears to hold the place of boundary between the two series of classes.

The distribution of the stars of the B.D. fails to correspond to the configuration of the Milky Way in three further particulars. The well-known fissure in the latter extending from Cygnus to Sagittarius is not shown, the broader and narrower parts of the Milky Way do not coincide generally with similar regions on the charts, nor do its brightest parts agree with their richest regions.

These are but some of the chief points which are placed in evidence by the first series of M. Stratonoff's charts. Charts 10 and 11 deal with another and if possible an even more interesting question—the distribution of the two first types of stellar spectra. Unfortunately the material as yet available is insufficient for satisfactory treatment, and it is further doubtful whether the method which M. Stratonoff has employed is quite the best for the handling of the facts within our reach. The chief features that the two charts show are the strong condensation of first type stars in Cassiopeia and Perseus, and of second type in Hercules and Cancer.

The twelve remaining charts deal with nebulæ and star-clusters, and here both hemispheres are shown. The nebulæ are first shown in general, and then the faint nebulæ, the bright, the small, and the extended are each shown in separate charts. Finally the star-clusters are represented. The chief fact which these later charts reveal is the one, now long recognized but of the first importance, that the nebulæ in general, and in each of the four classes above named, avoid the Galaxy and cluster towards its poles; whilst the irregular star-clusters—the globular clusters being excluded—have the most striking affinity for it. The inference is clear that the Galaxy, the irregular clusters, and the nebulæ form but parts, complemental parts, of one and the same system, and that, broadly speaking, the heavens, as we behold them, form but a single structure.

It is not possible within the compass of a few paragraphs to indicate the full instructiveness of the figures and charts which M. Stratonoff has placed at the disposal of astronomers; the writer's purpose will have been fulfilled if the foregoing lines lead to their attentive study by all of those to whom the great problem on which they bear appeals. E. W. M.

NOTES.

Comet Notes.—A new comet (1902 *a*) was discovered by Brooks at Geneva, U.S.A., on April 15. It is described as of magnitude 8·5, circular, 3′ in diameter, with a central condensation and a tail less than ½° in length.

The following observations have been received:—

	G.M.T. d h m	R.A. h m s	N. Dec. ° ′ ″	Place.
1902 Apr.	15 21 53	23 8 8	27 25	Geneva (1).
	16 13 15	23 15 41·1	26 6 35	Königsberg (2).
	14 17·8	23 16 12·8	26 1 21	Copenhagen (3).
	14 46·6	23 16 27·6	25 58 59	Bamberg (4).
	23 52·2	23 21 7·7	25 12 26	Lick (5).

The daily motion deduced from (1), (2) is $+11^{m}\ 47^{s}$, S. 2° 2′·4, while that from the mean of (2), (3), (4), combined with (5) is $+12^{m}\ 19^{s}{\cdot}2$, S. 2° 2′·6.

Elements have been computed by Drs. Kreutz and Strömgren as follows:—

T	1902 May 7·16 Berlin M.T.
ω	228° 23′
☊	52 15
i	66 30
log q	9·6544

Ephemeris for Berlin midnight.

	R.A. h m s	S. Dec. ° ′		R.A. h m s	S. Dec. ° ′
May 5	2 24 53	7 57	May 17	3 16 45	7 37
9	2 46 25	9 7	21	3 27 59	6 4
13	3 3 15	8 47	25	3 37 37	4 19

The brightness on May 5 is equal to that at discovery, on May 25 it has sunk to ⅙. The comet will only be visible in the southern hemisphere in May. A. C. D. C.

Minor Planet Notes.—A new planet HW, of magnitude 12·5, was photographed by Wolf at Heidelberg on March 6 on a plate of the region of Nova Aurigæ.

A planet announced as HV proved not to be new, but identical with (72) Feronia. The planet HS, announced last month, proves to be identical with (359).

Prof. Wolf pays much attention to the recovery of missing planets by photography as well as the discovery of new ones. Among others (296) Phaëtusa, (393) Lampetia have been recently detected, also the very interesting planet (279) Thule, which was only of the 14th magnitude. On the other hand, (44) Nysa, (257) Silesia, (331), Etheridgea, (450) Brigitta, (468) were sought for without success.

Planet 472, discovered last year, has been named Roma.

A. C. D. C.

We are indebted to M. Bigourdan, of the Paris Observatory, for the following notice of M. Cornu:—

M. Cornu (Marie Alfred), Member of the Institute of France, of the Bureau of Longitude, Professor at the Ecole Polytechnique, Foreign Member of the Royal Society, Associate of the Royal Astronomical Society, &c., &c., succumbed to a short illness, which at first gave no indication of a serious nature.

A few days after Easter he set out from Paris, in good health, to spend a short time near Orleans, where he had made a large number of well-known observations, and where death overtook him on April 12, in his full vigour and talent.

Born at Chateauneuf (Loiret) on March 6, 1841, M. Cornu entered the Ecole Polytechnique in 1860, and two years later left it as Elève Ingénieur des Mines. From this time he was devoted to science, and commenced that numerous series of memoirs, remarkable for their clearness, for their classical perfection, and of which a mere list will be one of the best homages we can pay to his memory.

Amongst these works we recount only those relating to astronomy, for which science M. Cornu had a marked predilection.

His determination of the velocity of light, made definitely in 1874, between the Observatory of Paris and the Tower of Monthéry, gave more exact results than previous determinations, as shown since by the results of Michelson and Newcomb. About the same time (1873) he applied, in conjunction with M. Baille, the method of Cavendish to determine the mean density of the Earth, and arrived at a value (5·53) the precision of which is remarkable.

On the occasion of the Transit of Venus 1874, he reverted to an idea of Sir J. Herschel's of overcoming the achromatism of the object-glass by varying the distances between the lenses; applying this method to the large object-glass of the Paris Observatory, he obtained some fine photographs of the Moon at a time when it was extremely difficult—the marvellous rapidity of the present plates being then unknown. Later M. Cornu naturally became one of the most active members of the several congresses which laid down the lines on which the photographic chart of the sky was to be executed.

The work of M. Cornu on the spectroscopes was not less marked, his determinations of wave-lengths furnishimg a remarkable verification of Balmer's formula.

He studied successively the ultra-violet and infra-red portions of the spectrum; and he thought out a method to distinguish the telluric rays from the purely solar rays, a method remarkably elegant, founded on the Döppler-Fizeau principle.

He also devoted time to photometry, and in this connection he suggested the observation of a semi-eclipse of Jupiter's satellites in place of the doubtful observation of complete disappearance. Not being satisfied with the air-bubble level as sufficiently delicate where the utmost precision was required, he invented an apparatus capable of replacing this level in certain cases; he also studied the Variation of Latitude, and introduced a zenith-nadir instrument fully appreciated by the International Geodetic Association.

To a theoretical knowledge the most profound M. Cornu added a remarkable dexterity of manipulation; many of his researches were conducted with instruments made under his supervision, in his laboratory, or by his own hands; and he has added improvements to micrometers, spectroscopes, photometers, polariscopes, &c. Horology had attractions for him, and he devoted much time to the synchonizing of clocks, and so late as 1900 had studied the effect of terrestrial magnetism on the rate of a chronometer.

We must not forget to mention his scientific notices, models of their kind, by which he contrived to place his countrymen in possession of the results of the best workers in science. Many of these will be found in the Annuaire des Bureau des Longitudes.

The importance of his labours were fully appreciated; in 1878 he received the Lacaze prize of the Academy of Sciences, and the Rumford prize of the Royal Society; in 1878 also he was nominated a member of the Academy of Science, over which body he later presided; in 1886 he was appointed to the Bureau des Longitudes. Successively he became Foreign Member of the Royal Society of London and an Associate of the Royal Astronomical Society.

Science and his country have by his death suffered an irreparable loss, which will also be felt in England, where he had numerous friends. But our thoughts carry us above all to the desolation his departure has caused in his immediate circle. Still, Madame Cornu has some consolation in the knowledge that she has the sympathy of all who knew him.

THE VARIATION OF LATITUDE.—A suggestion of a new complication in the determination of the motion of the terrestrial pole is made by Mr. H. Kimura (Ast. Journ. No. 517). Taking the figures from Prof. Albrecht's report of the observations made at the various stations for the direct determination of the variation of latitude, he finds that smaller residuals are obtained if he introduces a term constant for all the stations but varying with the time. In effect, the suggestion is that the plumb-line at all these ten stations has an annual oscillatory movement north and south, the amplitude being nearly $0''\cdot03$.

A NEW SOLAR THEORY.—The following abstract of a paper by Dr. J. Halm, of the Royal Observatory, Edinburgh, is taken from the B. A. A. Journal:—

Previous theories to account for the periodic changes have taken no account of the absorbing envelope surrounding the photosphere.

If the loss of energy by radiation exceed the production due to shrinkage, the temperature must fall. The level of the layer of maximum radiation, *i. e.*, of the photosphere, must shift towards the centre, and consequently the photosphere becomes protected by a greater thickness of absorbing and reflecting matter. After a time the increasing reflection may overheat the photosphere, but the overheated material may be retained at the level of the photosphere by convection currents, until the upward tendency becomes so strong as to produce an eruption by which thermal equilibrium is temporarily restored, after which the cycle is repeated. The mathematical expression of the theory gives an equation from which a curve of sun-spots may be computed agreeing very closely with the results of observation, while a "great period" of solar phenomena is accounted for by changes in the intensity of the convection currents, the equation showing that when the spot development is powerful the rise from minimum to maximum will be accelerated. A remarkable conclusion from the theory is that times of maximum spottedness correspond to times of minimum radiation, which would seem to be supported by the more important recent researches. (Nat., Feb. 13.)

In No. 3783 of the *Astronomische Nachrichten*, Herr F. Krüger gives the colours of 328 stars which he has examined in the zones 20° to 40° North Declination. The magnitudes vary between 1·5 and 8·5. The following is abridged from his conclusions:—

Colour.	No. of Stars.	Spectral Types.				
		I.	II.	II. or III.	III.	IV.
W.....	8	2	5	..	1	..
G.W.....	31	3	21	1	6	..
W.G.....	149	1	72	4	72	..
G.....	110	..	37	2	71	..
R.G.....	24	..	4	..	19	1
G.R.....	5	..	..	..	1	4
R.....	1	..	..	..	..	1

We are glad to find that the Rev. E. Espin is devoting time to double-star work. In the *Astronomische Nachrichten* he publishes a list of 72 new pairs found in 1901. Thirty-five of these are under 5″·0.

The same number contains 109 new pairs discovered by Prof. Aitken at Lick. This list brings his total number of discoveries up to 312, the great majority being close and difficult pairs.

Mr. Espin's measures of the group Σ 484 show that there is very little, if any, motion *inter se*. The meridian observations of the two principal stars, Lalande 7430–7433 are unusually discordant, and although we may suspect a proper motion, it is by no means certain. Meridian observations ought to be secured.

A GENEROUS friend of Prof. E. C. Pickering, who desires to remain anonymous, has just put the sum of $20,000 at his disposal for the benefit of Harvard College Observatory. This munificence will at once relieve a most pressing need of the Observatory. It is proposed to use a portion of the fund in erecting a wing which will provide an adequate storehouse for the increasing collection of astronomical photographs. The remainder of the sum will be spent from time to time in studying new objects of interest on the photographs.

THE first part of the determination of the difference of longitude Paris—Greenwich is nearly completed, ten out of the twelve nights' observations necessary for the programme in the spring having been already obtained by the English observers. Mr. Dyson is now at Paris and Mr. Hollis at Greenwich. Unfortunately one of the French observers, M. Renan, has been taken seriously ill and has been replaced by M. Lancelin.

THE "Results of Rain, River, and Evaporation Observations made in New South Wales in 1899," which we have received from Mr. H. C. Russell, the Government Astronomer, are of the usual exhaustive character. The material is gathered from 1742 stations and indicates that 1899 was an exceptionally dry year.

THE *Annuaire Météorologique* received from the Royal Belgian Observatory at Uccle is as interesting as ever. M. Lancaster discusses the temperature, rainfall, and wind pressure at Brussels from 1833–1900. M. Vanderlinden gives a general idea of the problems connected with the upper atmosphere.

IN recognition of the fact that Prof. E. C. Pickering has completed 25 years' of service as Director of the Harvard College Observatory, the Staff of the Observatory have presented him with a silver cup.

THE death is announced of Prof. C. A. Bacon, of Beloit College, Wisconsin, and Director of the Smith Observatory.

THE next Meeting of the Royal Astronomical Society will be on Friday, May 9; of the British Astronomical Association on Wednesday, May 28.

FROM AN OXFORD NOTE-BOOK.

THE event of the last month for Oxford has of course been the publication of Cecil Rhodes's will. The University was in Vacation at the time and is only now reassembling, so that comments on that wonderful document have been few and scattered. But there can be no doubt that it means a great deal to Oxford. To take the simple fact that it ensures, independently of existing factors, the presence in Oxford of 200 new students (and perhaps, with their friends and acquaintances who may be attracted along with them, of 500), the endowment is almost in itself a guarantee of the permanent prosperity of Oxford as a University: a matter on which some who have noted the rapid rise of other Universities have not been altogether free from anxiety. The endowment is of course indirect: of direct endowment the University receives not one farthing. Benefactors are almost unanimous in avoiding that one species of benefaction which is most effective, viz. giving money to be expended on pressing needs. Oriel College has been happy in this respect, but the Colleges in Oxford and Cambridge (and London) are corporations practically independent of the University—a fact which is seldom comprehended by those not familiar with these Universities. The difference is a vital and most unfortunate one for the University, which remains poor in the presence of wealth. We have to fight for every penny expended on scientific equipment; while the Colleges show to the public all the signs of wealth. New College, for instance (the College to which I belong), has been in the last few years spending about £10,000 on renewing the pinnacles to its chapel; Magdalen College recently received a present of £5000 on condition that it was "not to be spent on any purpose connected with education," and decided to make a new roof to their dining-hall with it; and now Mr. Rhodes has contributed £100,000 towards the maintenance of the Fellows of Oriel College in "dignity and comfort," and simultaneously expressed merely a pious wish that Oxford may develop a good medical school. It did not apparently occur to him that the organization of such a school costs money. This sounds, I am afraid, like an ordinary grumble, but such is far from my intention. I regard the Will with admiration, and believe the bequests made may indirectly do more for the advancement of learning than a direct endowment; but it is inevitable that we in Oxford should feel the full force of the considerations above indicated.

ONE of the daily papers printed an amusing story *à propos* of Mr. Rhodes's estimate of the financial abilities of the Fellows of Oriel:—

> Although Mr. Rhodes thought that in financial matters the "dons" of Oxford Colleges were mere children, some of them display an ingenuity in bookkeeping capable of making an ordinary millionaire envious, as will be seen from the experience of a correspondent who forwards the particulars. He used to be a member of a comparatively poor college, whose authorities determined, in order to make both ends meet, to collect an "establishment" charge from its alumni, which was calculated at so much per cent. on their "battels." During the whole of one term this gentleman was "down," and at the end he duly received a bill in the usual form, which read something like this: Tuition, £0 0s. 0d.; buttery (provisions, &c.), £0 0s. 0d.; fees, £0 0s. 0d.; and various other items were enumerated in regular order, all marked with the three noughts. Then, at the end was the entry, "To interest on the above (establishment charge), £12." Our correspondent thinks that the "children" who invented interest on this scale are endowed with a financial genius bordering on the sublime.

In striking contrast with the methods of Mr. Rhodes's Will are the Carnegie endowments for research. The proper spending of these will be a matter of considerable anxiety and difficulty: no doubt many schemes are already afloat. I wonder whether it would be possible, under the terms of the trust, to use a portion of the funds for producing scientific treatises of a more or less popular kind? Not the purely popular book which sells well and can take care of itself: but a scientific account of the state of some line of research, written by an authority, for the information of those working at allied subjects who cannot find time to digest the original memoirs. A sort of glorified text-book, if you like: collecting together the scattered threads of a subject. Such books would be worth any number of "International Catalogues of Scientific Literature." They appear occasionally even now, in spite of the fact that they do not pay. Why should they not be subsidized? even handsomely? There are many worse ways of endowing research.

The appearance of the *Nautical Almanac* for 1905 reminds us of the important solar eclipse of August 30 in that year, which has a maximum duration of $3^{m}\ 44^{s}$. One end of the line is in Labrador and the other in Egypt; and there is a difference of about 2½ hours between the times of totality at these places, so that we shall have an excellent opportunity for detecting possible changes in the corona if the ends of the line are properly occupied. It will be hot in Egypt no doubt, but (I gather) not unbearably hot: for Labrador August should be a pleasant month. No doubt most observers will go to Spain. One remark suggests itself: this is the only serious eclipse in a rather long interval of ten years, and may be accorded therefore full attention. Would it not be a step in advance to organize preliminary expeditions, a year or so beforehand, to the selected stations, in order to set up the apparatus with due regard to all the conditions?

There is sometimes a virtue in repetition; and I will repeat the expression of gratitude formerly offered to the editors of the *Astrophysical Journal* for the promptitude with which they have again published Mr. Ritchey's beautiful drawings of the nebulosity round Nova Persei. The March number contains 5 of these diagrams, showing the changes from 1901 September 20 to 1902 February 8. We are promised shortly "measures and a discussion of the entire series of negatives, including those of March and April (after which time this object will be inaccessible for photographing for several months)": but the provident will secure a copy of this number of the *Journal* in any case. The changes are beautifully shown: *apparent* changes Mr. Ritchey is careful to call them, reserving his opinion whether or not there is actual motion of matter. But he adds this significant sentence:—

> The rapid fading of parts of the principal ring, and the sudden brightening and rapid outward movement of parts of the outer ring, . . ., are phenomena which strongly support the theory suggested by Kapteyn and others, that the apparent motion is due to changes of illumination of a stationary nebula.

We are very sorry to learn that Mr. Ritchey has been ill, and hope that when the news reaches us the actual catastrophe, like that in *Nova Persei*, is a thing of the past.

THE following answer from a candidate under stress of examination may serve as a warning to some lecturers:—

> The distance stars are away from us is inconceivable. The mere figures in miles does not convey any meaning to us. To get any idea we have to take simple illustrations, which illustrations do no good except fill us with awe.

The futility of the whole campaign announced in the last words reminds me of an essay on beer which was produced by a small friend of mine many years ago. An elaborate description was given of the making:—"First you get hops, and then you get malt, and then you mix them," and so on, following closely the lesson obviously learnt, and leading up to the following conclusion:—"And so the beer is made; and it is bad for you if you drink it." I wish I could remember more of the essay: it seemed such a lot of trouble to take only in the end to be "bad for you if you drink it"; and similarly the former essayist assures us that after a lecturer has gone through the toil of preparing simple illustrations of the distances of the stars they "do no good except fill us with awe." I must remember this in future lectures. About the beer, I'm afraid I don't share the essayist's opinions.

THE mention of lecturing reminds me of the following cutting from *Punch* of March 9, 1895, which is not of any great merit, and has been lying in the drawer for a suitable occasion which does not seem to present itself. It must go in as a mere miscellaneous item, *à propos* of nothing.

IRISH ASTRONOMY.

Sir Robert Ball, recently delivering a lecture (by request) under the above title, admitted that he did not quite know what it meant, as he did not suppose Irish astronomy was different from that of other nations. Isn't it be jabers? Judging by parity of reasoning, we can imagine that Irish astronomy may be as *sui generis* as are Irish politics. It is probably unusually nebulous, and characterised by the revolution of suns round their satellites, and the prevalence of excentric comets and shooting stars. Had Addison had it in his mind, he would probably have written his celebrated hymn somewhat as follows:—

The spaycious firmament on hoigh,
And all the green Hibernian skoy,
And wrangling hivens a foighting frame,
The reign of chaos do proclaim.

What though the "stars" do shoine—and squall,
And on each other's orbits fall!
What though no order, stable, sound,
Amidst those jarring sphayres be found!
Onraison there doth loud rejoice,
At hearing echoed her own voice;
For iver shouting as they shoine,
Our hiven's a Donnybrook divoine.

ANOTHER item which has been in hand for some time is the following, sent by a correspondent. The reason of the delay is that in sending it the author was not very sure of himself; much might be said on the other side, he thought, and perhaps I could embody it in a more impartial statement. But it seems to me after all better as it stands, remembering the author's admission. The incident about Gauss is to be found in a letter, No. 1263 of the Gauss-Schumacher correspondence; and this is the paragraph exact as sent:—"We see much about 'Laplace's Nebular Hypothesis' in the speculations of popular writers on astronomy. They have heard of both hypotheses mentioned in the title, and proceed like the writer who crammed on China and Metaphysics from the encyclopædias, and combined the information in an essay on Chinese Metaphysics. It is not certain that Laplace himself would know what these writers refer to, and he might ask if they had ever read, at first hand, what he said in his exposition of his form of the older hypothesis, restricting it to the evolution of the planetary bodies of our system from a contracting solar *atmosphere.* At any rate it is certain that Gauss, who knew something about astronomy and its history, confessed ignorance as to what was meant by 'Laplace's Nebular Hypothesis' when he met with the phrase in 1849. The nearest he got to an explanation was a reference in an English popular work on astronomy, where, however, he says he finds no information, since the book tacitly assumes that the reader knows about it. He says 'if I had time to hunt I would find the real source'; but he slyly adds, 'what is probably meant is "Laplace's Hypothesis" (which, however, was propounded long before him), &c.' Very likely some of the writers who use the phrase may think Gauss over-punctilious as to correctness in ideas and expression, and that an attempt on their part to emulate him in that regard might embarrass them in their own speculations on the subject. They will doubtless sympathize with the student who could not see the difference between $2a$ and a^2. The teacher spent an hour in patient explanation of this recondite point and finally exclaimed in despair, 'Well, *now* do you understand the difference?' 'Yes,' hesitated the poor boy, 'I think I see there *is* a difference, but, Professor, don't you think it is an *unnecessary refinement*?'"

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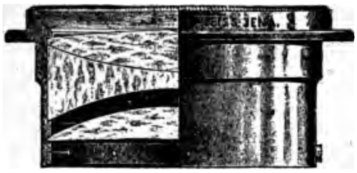

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EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXV. JUNE, 1902. No. 319.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 May 9.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. Seventy-two presents have been received since our last Meeting. Among them are two which I might specially bring to the notice of Fellows:—Sir Norman Lockyer's Account of Observations of the Total Eclipse on 1900, May 28, presented by the Solar Physics Committee, and 63 enlargements of Astrographic Chart Plates, presented by the French Ministry of Public Instruction.

A vote of thanks was passed to the Donors.

Mr. H. C. Plummer read a paper "On the Accuracy of Photographic Measures: Second Note." He referred briefly to the work which had been recently done on the subject and the circumstances which had led him to write a second paper. The present note was mainly devoted to a consideration of two memoirs recently published by M. Loewy. These seemed to call for some criticism both as to the material used as a basis for the research and also as to the general method employed in the discussion. As regards the material, too much weight had apparently been attached to measures effected on a single plate. Such measures might possess an exceptional character, and could not be regarded as a satisfactory foundation for an elaborate numerical theory. It also appeared that the measurers had not gained adequate experience of their work and that their results had not been kept perfectly distinct. In fact some series of measures which should have been made by one observer were, in his absence, made by another, and thus evidence was really wanting to decide the question of the relative advantages to be gained by changing the observer and by simply changing the orientation of the plate. Exception might also be taken to the method adopted by M. Loewy. His object had been to find a general numerical formula which would give a superior limit to the probable error of a co-ordinate determined photographically under any circumstances. It was easy to devise a formula such as

$$\epsilon^2 = \frac{\epsilon_1^2}{n_1} + \frac{\epsilon_2^2}{n_2} + \ldots$$

by assuming a number of sources of error, and so to connect the resultant probable error ϵ with the component probable errors $\epsilon_1, \epsilon_2 \ldots$ And it was not difficult to determine the constants of such a formula by varying the conditions of observation. But in the first place, probable errors could rarely be determined with any great accuracy, and consequently an inevitable accumulation of numerical error would lead to a fallacious result. In the second place, the formula could not hold unless the sources of error obeyed certain laws. In particular it was necessary to be assured that the sources of error were really independent, and in every case it was necessary that positive and negative errors should be equally likely to the same extent. Now in a former paper, he (Mr. Plummer) had shown that the errors did not possess the first property, for notably improved results were obtained by combining measures effected in opposite orientations of the plate, a fact which revealed the existence of compensations in pairs of measures. Equally he now found in the measures of a plate given by M. Loewy in a third memoir evidence of pronounced personality. This equation, the reality of which was apparently denied by M. Loewy, was actually of such a magnitude as practically to control the sign of whole series of measures in several cases.

Mr. Hinks. I should like to be allowed to make a few remarks. The fundamental question at issue between M. Loewy and Mr. Plummer appears to be the old question as to whether there really is any personality in making measures on a photographic plate which can be got rid of by reversing the plate. (Blackboard.) When M. Loewy undertook the discussion of the kind of results we should be likely to get out of the measures of the Eros plates he raised these very elaborate and considerable formulæ upon the assumption that there was no such compensation. And that fact strikes one at once. Mr. Plummer immediately took up the question from the point of view of proving that there was compensation, and got results from M. Loewy's figures that showed a very notable effect as the result of reversing the plate. At this point, unfortunately, I thought I could add to the weight of evidence in Mr. Plummer's paper, and I prepared a small note which was read at the December Meeting of the Society, and which, I am sorry to say, contained a most glaring fallacy. It has been pointed out to me by a friend at Cambridge, and it invalidates my argument. Since then M. Loewy has been examining the matter afresh from Mr. Plummer's point of view, and he finds that there is a certain amount of compensation, but he will not admit anything like the amount of compensation that Mr. Plummer suggests. M. Loewy finds a diminution in the probable error of about 10 %, and Mr. Plummer finds a diminution, I think, of about 40 %. M. Loewy, when he saw Mr. Plummer's result, felt sure that Mr. Plummer must have dropped a $\sqrt{2}$ somewhere or other, and someone else is also of opinion that there must have been this factor dropped; for the curious thing is that if you multiply Mr. Plummer's result by $\sqrt{2}$ it comes out to very nearly M. Loewy's. I called Mr. Plummer's attention to this, but he has re-examined his work and refuses to admit that there is any such factor dropped out. The fact remains, however, that M. Loewy finds there is a small compensation on reversal, and Mr. Plummer says it is a large one. Therefore someone else will have to go through all the figures again *.

I had the curiosity to re-measure some plates I took last summer in order to see whether, after eight months, one does make the same estimate of the centre of the photographic images as one did before. And the result of the re-measuring of those plates was that I came to the conclusion that one did not. When the work is repeated after some time, one does set upon a different point as to the centre of the image. That being so, it follows clearly enough that a reversal of the plate need not be necessarily effective unless you have an instantaneous reversal. It rather supports M. Loewy's view that reversal cannot be accepted as a complete solution of the difficulty. I also found in looking over my measures that there was extremely little evidence of compensation. I am much more inclined to think that M. Loewy is practically right—that there is very little compensation when you reverse the plate. That being so, the question of what the proper procedure is becomes a much more difficult one, because supposing there is no compensation unless you reverse instantaneously—and reversing instantaneously is possible, but it is not a very nice thing to do—if you measure again in the ordinary way your judgment as to the centre of the image will have altered. So the point comes in, is it worth while to re-measure your plates at long intervals? I think, obviously, it would be, supposing your plate were perfect, and supposing the star-image on the plate corresponded exactly with the position of the star in the sky; it would

---

* *Note added May* 17.—In reply to this suggestion that a numerical factor had been omitted, Mr. Plummer has sent me a part of his numerical work which makes it perfectly clear that there is no truth in the idea, and that the source of the discrepancy between his results and those of M. Loewy must lie deeper, in the theory of the matter. I am glad to have this opportunity of withdrawing the mention of a suggestion—it was not my own—which has, I am afraid, caused Mr. Plummer some annoyance.—A. R. H.

be worth while spending a lot of time on it. I think the best thing to do would be to make a single setting on the stars on one day, another setting on another. It seems to me that things are rather tending towards taking a large number of images and making a single setting for each image; not reversing the plate for each image, but measuring half of them in one orientation and half in the opposite. The evidence of M. Loewy is—and it is strongly confirmed by Prof. Kapteyn—that one ought to spend very little time on any particular images, but ought to take a lot of images. One important point that M. Loewy brings out, which seems to have been overlooked before, is that a series of exposures on a plate should not run parallel to the réseau lines, but diagonally.

Mr. Plummer. I may be allowed to explain that there is no obstinacy on my part in this matter. If anyone will point out precisely where there is any doubt with regard to the statements in my paper I should be glad to go over it, and should be happy to make any necessary correction, but the difficulty is that no one has put his finger on a point where it is wrong.

Prof. Turner. The main point is how much time should we spend on a single photograph. A natural temptation is to exhaust the possibilities of one plate before we pass on to another, but I submit that that is not actually the best method. We ought to look at the total amount of material, to settle how much time ought to be expended on it, and distribute this amount of time fairly over the material. That seems to me to be a more reasonable procedure than examining each plate in an elaborate way. While I admit the great value of M. Loewy's investigations, I think that Mr. Plummer has shown the weakness of spending too much time on one plate. Very little is gained by repeating measures in more than two orientations. I would venture, as a proof of the danger of an excessive programme, to point to the very small amount of work which seems to have been done on the Eros plates. If more rapid processes had been adopted I think the measures would have been performed by many observers who have been frightened by the enormous amount of work it is suggested they should do on each single plate.

Mr. Dyson agreed with what Prof. Turner had said, as to the necessity of the reversal of the plate. An additional advantage was that blunders were avoided by repeating the measures. He believed that most observers had some personality, but that it could be eliminated if the observer tried to do so.

The Astronomer Royal. During the preparation of the Astrographic Catalogue a good deal of material has accumulated bearing on this question, which is conclusive to our minds in settling this question of personality, and also the accuracy that might be expected from any one plate, or the measurement of any one image. In the first place, the personality of the various observers has been determined by comparison *inter se.* There were five observers engaged in measuring, who had had sufficient practice to have their personality well determined in the different positions—right and left and up and down; and the result of the discussion was, so far as I remember, that in nearly every case personality was practically eliminated by reversal. As the result of that, the practice was adopted of always having the plate measured in two positions—direct and reversed—by the same measurer. In the case of the astrographic catalogue there are two images measured. The first with 6 minutes' exposure is measured by one measurer in direct and reversed positions of the plate at different times, and the other, having three minutes' exposure, by another; and on the whole the results are very satisfactory, and the personality of measurement seemed to be very steady, and, as has been said, it is practically eliminated by reversal. Then there is the question of what degree of accuracy can be got in the measures of a single photograph. To determine that we had a photograph of one field measured by each of the five different measurers in the two positions, and then we had five photographs of the same field measured in two different positions by one observer. The conclusion we came to was that there is a very large portion of the error of measurement that is inherent in the plate, so to speak, and that practically it cannot be eliminated, however many measures are made of the same photograph. It can be reduced by measuring two images, but there still seems to be some systematic error in the plate. There is just one other point I would like to mention. It will be seen that I am in accord with Mr. Hinks and Mr. Dyson on the question of taking a number of images on a plate. In the case of the Eros plates we arranged to take four images on each plate, and also another image displaced considerably to give orientation, and to give measures under different conditions of the field. We had thus five images on the plate, and we took as many plates as we could get in the night with two instruments. With reference to the possibility of bias from having the images in a line, which M. Loewy calls attention to, I should explain what the system has been at Greenwich. We have a square formed by the wires in the field of view of the guiding telescope, and for the exposures on Eros the guiding star was placed at each of the angles of the square, so that there were four images formed. In using this square, care has been taken that these images should not interfere with each other to any appreciable extent, and I do not think that the observer is at all biassed by the different images. Then, as I said, another image was taken by putting the clock out of gear for a few minutes, so the image was taken at some distance from the centre of the field.

There is another paper to-night, on the Satellite of Neptune with measurements of photographs which Mr. Dyson has undertaken, which throws some light on the limit of accuracy which can be obtained from any one photograph. It will be seen from it that nothing can be gained by making a large number of measures on a single photograph.

Mr. Plummer. With regard to the question of personality, sometimes peculiar cases may arise. We have had such cases at Oxford, and personality is certainly not a thing that will always disappear with increased experience, because there is one case on record in which personality was increasing as the observer became more experienced, and we had another case in which a very experienced and skilled observer would show suddenly a large personality without any warning, and yet afterwards return to a normal state of things. I am in thorough accord with those who say it is not worth while to make too many measures on a single plate; that is precisely the point which I have been trying to urge.

Father Cortie. The present paper, on "The Spectrum of Nova Persei, 1901, on and after September 5th," is possibly the final paper on the subject which will be presented by Father Sidgreaves, who has observed the star from February 28, 1901, to February 12, 1902, securing in all 95 photographs of its spectrum on about sixty nights. Remembering that these photographs were taken for the most part with an objective prism of only 4 inches aperture, in combination with a telescope of the same aperture, we can appreciate the brilliancy of the spectrum of the star, and also, may I add, the painstaking zeal of Father Sidgreaves in following the star for nearly a year. The chief points of the present paper are the remarkable structure of the two strong violet bands at w.-l. 3969 and 3869, the permanence of this structure until the star became too faint for the Stonyhurst instruments, and the permanence in width of all the bands in the spectrum. In the picture on the screen it will be noticed that there are four well-marked divisions in the two bands which increase in width and intensity with increasing wave-length, giving a series in each band of five bright lines, the similarity of structure suggesting similarity of origin. The first division at the less refrangible head of the band is quite distinct in these enlarged photographs from the originals, and was only brought out clearly by them, although it was suspected in the originals. The process of enlargement was first to make on bromide paper an enlarged picture of the original negative, then to stop out any pin-holes or flaws that might possibly give spurious lines when the cylindrical lens was employed, to reduce this enlarged picture and then again enlarge by means of the cylindrical lens. In spite of all precautions, however, there are one or two spurious lines in Hγ. The same structure which is seen in the two bands at w.-l. 3969 and 3869 can also be seen in the band 4363 in photographs taken with the direct-vision prism at the eye-end of the 15-inch telescope. In the same photographs there are also divisions in the lines at 4960 and 5007, but the scale of the photos is too small to allow the assertion that the same structure is shown as in the lines 3969 and 3869. It might possibly have existed. We have therefore as a permanent feature in the spectrum of Nova from September 5, 1901, to February 12, 1902, this structure in the lines or bands. But Ritchey's and

Perrine's photographs show indubitably that the Nova has become a nebula. The ordinary spectrum of a nebula is one of thin bright lines. Here, then, we have the enigma of a spectrum which is nebular as far as the positions of the lines go, but totally unlike the ordinary lines of nebular spectra. This is no new feature in the later spectrum of a new star. It was observed also in the later spectrum of Nova Aurigæ by Sir William Huggins. We have on the screen a photograph of his drawings taken from the Proc. R. S. (vol. lvi. 1893, p. 32), and in this picture we see structure in the bands at 4960 and 5007, just as Father Sidgreaves's photographs bring out the same appearances in the same lines, and more especially in the two violet bands of Nova Persei. The other noticeable point is the width of the bands, which has remained also constant for the six months September 1901 to February 1902, and between 20 and 30 tenth-metres. This feature also occurs in the hydrogen lines, which were almost as wide in February 1902 as they were in February 1901.

Dr. W. J. S. Lockyer. I should like to say that in the earlier spectra of Nova Persei one of the most striking appearances was that the lines of hydrogen were not seen single. It was found that they were split up at least into three parts, and from these divisions of the hydrogen line it was generally assumed that it was of a structural character, and that the displacement from their normal positions indicated that motion was present. In fact, the width of the line as a whole was put down to the motion of the different masses of the hydrogen particles. It is most interesting to find from the later photographs that the nebular lines exhibited a similar structure. Probably, in the earlier photographs when the hydrogen was beginning to wane and the nebular lines were getting brighter, there was not sufficient density in the photograph to determine this structure in the nebular line, but now that the hydrogen has diminished very considerably, and the nebular lines are the most prominent features of the photographic spectrum, this structure has been observed, and I think that this is a very important fact, because it is exactly what would have been expected from the previous behaviour of the hydrogen lines. I think the remarks in the paper are most interesting, and those who have photographs of the spectrum of the Nova since it became a nebula will look to them and see if it corresponds to what has been shown on the screen *.

Professor Alex. Herschel. I should like to ask Father Cortie if either of the pair of compound ultra-violet nebular lines which have been described in the Nova's spectrum showed any apparent agreement with the strong fluted ultra-violet carbon band at about 3850–80, which seems to be near one of them. Although the light of the

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* I may add that the photographs of the Nova secured at the Solar Physics Observatory, Kensington, have shown a similar structure, an account of which has already been published (Proc. R. Society, vol. 69, p. 136).—W. J. S. L., 1902, May 17.

Nova is known to be rich in ultra-violet rays, which act most powerfully on photographic plates, and although several fluted carbon bands are known to be situated in that spectrum region, I have not heard of identifications of any of them having been obtained with bright bands in the spectrum of the Nova; and I should like to know if the most adjacent looking one of the Nova's close pair of ultra-violet nebular lines to the best known and brightest of those ultra-violet carbon bands showed any sufficient similarity, by near agreement in position with it, to suggest the possibility of a carbon origin.

Father Cortie. I cannot answer Professor Herschel's question with regard to the carbon band, but in the last paper by Father Sidgreaves (*Monthly Notices R. A. S.*, Dec. 1901, p. 153) he will find a table which gives the probable origin of all the lines observed. If I remember rightly, Father Sidgreaves was at first inclined to believe that one of these two violet bands was due to cyanogen.

Father Cortie. The sun-spot group of May and June 1901 was a very large one for a minimum epoch of sun-spot activity, and, moreover, was the only sun-spot of any size that had been seen from the previous November. The first table in the paper now presented gives for each of the months November 1900 to June 1901 the mean daily number of sun-spot groups and the mean daily number of faculæ derived from the Stonyhurst drawings, the gaps in the drawings being filled by excellent direct eye-observations of Mr. Hadden, of Alta, Iowa, U.S.A. The quiescent state of the Sun is brought out by the table, although at the same time it shows a gradual progression in the numbers of the faculæ from January to June. But the sun-spot group is of unique interest on account of its having appeared one day after the total solar eclipse, and in the same position as Mr. Perrine and the Greenwich observers have shown as a very remarkable disturbed area in the corona. But besides the identity in position, the Stonyhurst drawings show that the disturbed area of the solar corona of of May 18, 1901, marked the time of the actual birth of the spot. From a study of the types or phases through which sun-spots pass in their life-histories it is possible to give, at least in the beginning of their lives, a very accurate indication of the time of the spot's birth, even though, as in the present case, it was born on the invisible side of the Sun. Before any knowledge of the coincidence in position of the spot and coronal disturbance could have been attained, it was stated, in a note to the *Observatory*, that the spot had been first seen at Stonyhurst on May 20, and that it was then only two or three days old. The next point discussed in the paper presented is that, though the spot was born about May 18, most likely actually on May 18, yet it occurred in a region of the solar surface which had been disturbed for at least a whole rotation of the Sun before the birth of the spot. Not only was there, as shown before, a progressive increase in the number of faculæ from January to May, but besides in April the regions north of the solar equator were disturbed by the appearance of veiled spots, and on April 21 a bright patch of facula appeared on the E. limb in the very position of the subsequent spot-group. Its character showed it to be new faculæ, as it was compact, small, and bright. Most likely, too, we have here another well-marked instance of a facula preceding the first outburst of a spot. The third point discussed is as to the connection of magnetic storms with this the one great spot of a quiet period. There was no connection, the magnets being either quite calm or only slightly disturbed when the spot crossed the visible hemisphere during its first appearance. At its second appearance in June, when it was much reduced in size, there were two moderate disturbances. As a set off, there were also two moderate disturbances in July, when the Sun's surface was quite calm. The fourth point discussed is the spectrum of the spot, which was observed very carefully with high dispersion, in excellent seeing-weather, between C and D. The most widened lines were the faint lines due to vanadium and titanium, to which reference has been often made in previous papers. Besides this picking out of the most widened lines in the region C to D, a special study was made of selected groups of lines. Sixty-one lines were observed in all, and a table gives their estimated widenings, their origins, their intensities in the solar spectrum. If we collect together the results and take averages for the lines of different metals observed, we find that the progression of the mean intensity of these lines in the ordinary solar spectrum is almost opposite to the progression of their mean widening. The vanadium lines are the most widened, and they are the faintest in the solar spectrum. The faint lines of titanium are also among the most widened, while the thick dark lines of iron and calcium are only slightly widened. This observation corroborates therefore what I have stated before, that at least in the red end of the spectrum at all periods of solar activity the most widened lines are faint solar lines, principally due to vanadium and titanium. It is worth noticing that these two metals are among those which, by a similar system of averaging, Mr. Evershed, in a recent paper, has found to be bright in the flash and faint in the ordinary dark-line solar spectrum. It would seem to follow that the level of sun-spots is the level of the upper more diffuse gases which give the flash-spectrum in total solar eclipses.

Dr. Lockyer. I should like to make one or two remarks on Father Cortie's observation. I think he stated that the spectrum of the sun-spot practically consisted chiefly of titanium and vanadium, and I think he said that sun-spots, as a rule, always consisted chiefly of these substances. A great deal of work has been done on sun-spots at South Kensington, and it has been found that there is a very wide difference between spectra of sun-spots depending upon the period of the Sun's activity. In fact, we have curves to show that in the last two maxima—1884 and 1894—that spot-spectra consisted chiefly of unknown lines, and that at minima the lines changed, and we obtained a spectrum of known lines, chiefly those of iron. As to the table written by Father Cortie on the board, I should like to know if he refers to the percentages of iron. We all know that the spectrum of iron consists of a great number of lines, and that 19 lines might be a very small percentage of the number of lines, and, on the other hand, the lines of titanium and vanadium might be a very great percentage. Perhaps he will let us have the information. Another point of interest refers to the intensity of the widened lines in the solar spectrum. If we take an ordinary arc line of iron in the solar spectrum to be widened, and compare it with its actual widening in a sun-spot, although the widening is clearly shown, its amount is not considerable in relation to the original line in the solar spectrum. It is far more easy to observe the widening of very thin lines, and it often happens that one can observe widened lines in the sun-spot spectra that are not found in the solar spectrum. I think it is far easier then to observe the widening of faint Fraunhofer lines than the strong lines: in fact, the strong Fraunhofer lines are not very much widened as a rule. I should like Father Cortie to answer my point raised concerning the percentages, and to know if iron is so predominant as he says. It is a curious fact that within the last two years, that is about sun-spot minimum, the observations at Kensington have been different from those made at any other minima. At other minima we have always had a great number of iron lines, but now we have practically very few iron lines, but mostly vanadium and titanium, which seems to show that the minimum at the present time is hotter than the previous minima.

Mr. Fowler. I think Father Cortie attached some special significance to the occurrence of lines of titanium and vanadium in the flash- as well as in the spot-spectrum. There is no doubt that titanium is a very prominent substance in the flash-spectrum; but as to vanadium, I think there is some uncertainty, and it may be advisable to take the precaution to examine into that before drawing any important conclusion as to the respective levels of the spots and flash-stratum. The whole paper is of very great interest, and I think it will be a very valuable contribution to our knowledge of sun-spots.

Father Cortie. With regard to the presence of vanadium in the flash-spectrum, I rely upon the authority of Mr. Evershed. Vanadium is one of the metals given in his paper under Group II. which are bright in the flash-spectrum and weak in the solar spectrum. With regard to Dr. Lockyer's question, I have not given the percentage of the iron lines observed to the total number of iron lines in this part of the spectrum, but only the mean widening of the iron lines actually observed in restricted portions of the spectrum picked out for special study, after the whole spectrum between C and D had been passed in review to see which were the most widened lines of all. My method of observation, therefore, differs from that practised at South Kensington, where only the six most widened lines in the part of the spectrum studied are observed. I am quite aware that a line faint in the ordinary spectrum is more easily observed when widened than a thick dark line of the ordinary spectrum, but still there is no difficulty in observing the widening of the broader lines. I quite admit, too, that in minimum sun-spots the iron lines are more affected than in maximum sun-spots; but underlying this variation, sun-spots have a permanent spectrum, and had I followed the South Kensington plan and observed only the six most widened lines in sun-spots between C and D, they would have been always the same. This paper is, in this respect, not an isolated paper, but the results agree exactly with those which appeared in a memoir on this subject which I presented to the Society in December 1890, and also further back with the results derived by Father Perry and myself from the comparative study of the spectrum of a spot observed in 1889 with one observed in 1884 (*M. N.* xlix. 8 June, 1889). With regard to the amount of widening expressed in estimated tenths of the normal width of the line the results are approximate only, a better division would perhaps be most widened, more widened, and widened lines.

Mr. Dyson then read a paper on photographic observations of the satellite of Neptune. During the last opposition fifty photographs had been obtained at the Royal Observatory with the 26-inch refractor which had been measured, and the photographs showed both Neptune and the satellite so well that very accurate measures could be made from them. The principle of the occulting shutter which was used has been explained to the Society by the Astronomer Royal. The photographs were taken on rapid plates, because the satellite was very faint, and for the purpose of determining the orientation the clock was stopped and short supplementary exposures were given. The exposures were of such a length that the images of the planet were comparable in density with those of the satellite. Exposures of 2, 4, 5, and 6 seconds were given on one plate. A comparison of the satellite with the supplementary images of Neptune gave the measure of its magnitude. On March 3rd it was found that 15 minutes' exposure of the satellite gave an image the same size as a 2½ seconds exposure of the planet, and 20 minutes on the satellite gave an image of the same size and density as 3 seconds on the planet, the mean showing that the planet requires 400 times the exposure of the satellite to give an equal image. The main purpose of the photographs was the better determination of the orbit of the satellite, and especially the mean distance, for the purpose of giving the mass of Neptune. In visual measures, owing to the difference of the magnitude of the planet and the satellite, it was difficult to avoid personality, or to be sure that it was avoided in all cases. The accuracy obtainable from the series of 50 photographs should be such as to give the distance of the satellite from the planet to one or two hundredths of a second. Four measures of the distance were made and an equal number of the position-angle, and the whole of it was done by three measurers.

Major Hills. I should like to ask a question on one point, and that is with regard to the method of determination of the magnitude. The assumption that in a photograph of objects of different light-intensities the resulting density of the deposit is directly proportional to the product of the intensity into the time of exposure does not appear to be justifiable. Abney and other experimenters have shown that for large differences of intensity, such as you are here dealing with, the relation is not a linear one.

Mr. Dyson then read a paper by *Mr. P. H. Cowell* on Reduction of Extra-meridian Observations of Planets, which had been communicated by the *Astronomer Royal*; also a paper by the *Rev. T. Espin* on Micrometrical Measures of Double Stars with the 17¼-inch Reflector; and *Mr. Whittaker* read a paper by *Mr. R. T. A. Innes* on Jacobi's Nome (q) in Astronomical Formulæ, with Numerical Tables.

The following papers were announced and partly read:—

R. T. A. Innes. "Jacobi's Nome (q) in Astronomical Formulæ, with Numerical Tables."

E. F. J. Love. "Series in the Nebular Spectrum, and in the Bright-line Spectrum of Nova Persei."

Rev. W. Sidgreaves. "The Spectrum of Nova Persei, 1901, on and after September 5."

Rev. A. L. Cortie. "Visual and Spectroscopic Observations of the Sun-spot Group of 1901 May 19–June 26."

P. H. Cowell. "Reduction of Extra-meridian Observations of Planets." (Communicated by the *Astronomer Royal.*)

Rev. T. E. Espin. "Micrometrical Measures of Double Stars with the 17¼-in. Reflector."

H. C. Plummer. "On the Accuracy of Photographic Measures: Second Note."

Royal Observatory, Greenwich. "Photographic Observations of the Satellite of Neptune."

The following gentlemen were elected Fellows of the Society:—

A. E. Conrady, F.R.M.S., 104 Park Street, Camden Town, N.W.
H. T. Gerrans, M.A., Worcester College, Oxford.
M. E. J. Gheury, 23 Downshire Hill, Hampstead, N.W.

The following Candidates were proposed for election as Fellows of the Society:—

H. C. Hardman, Ruskin College, Oxford (proposed by Sir R. S. Ball).

F. K. McClean, 1 Onslow Gardens, South Kensington, S.W. (proposed by Frank McClean).

Rev. E. A. Phillips, B.A., the Training College, Exeter (proposed by Arthur W. Clayden).

THE BRITISH ASTRONOMICAL ASSOCIATION.

G. M. Seabroke, F.R.A.S., *President*, in the Chair.

Secretaries: A. C. D. Crommelin, and
E. W. Maunder *vice* J. G. Petrie.

The sixth Ordinary Meeting of the British Astronomical Association was held at Sion College on Wednesday, 30th April. The election of seven new Members was confirmed, and the names of two Candidates for admission were read and suspended.

Mr. E. Walter Maunder said the Council had decided to revise the lantern-slide collection and bring it up to date, and a Committee had been appointed for the purpose, and he appealed to the Members for advice and assistance. The opinions of borrowers as to deficiencies in the collection would be valuable, and helpers were wanted in the work of making new slides. There were two distinct departments—one, the making of negatives from which slides could be made, and the other the making of the slides themselves. The work of revision was tolerably heavy, as the Association had something like 3000 negatives and 2000 slides in their collection. No doubt many Members had interesting lantern-slides, and if they would give copies of them to the collection the Council would be grateful. They would be particularly glad to increase their collection of slides of private observatories. The lantern-slide department was closed till the end of September.

Mr. Maunder read a note from *Prof. Brooks*, of Smith Observatory, Geneva, U.S.A., respecting the discovery of the first comet of this year. The comet is moderately bright, telescopic, with a very minute stellar nucleus and a short tail.

Some astronomical photographs presented to the Association by Mr. Alexander Smith were then exhibited, including a fine view of the central portion of Orion and another of the Hyades.

Mr. Walter Goodacre (Director of the Lunar Section) gave the results of observations of the Total Eclipse of the Moon, April 22, 1902, by several members of the Section. The weather generally was cloudy and unfavourable. Mr. Goodacre then referred at some length to the astronomical work of the late Herr J. N. Krieger, who died recently at San Remo at the early age of 37 years. It was from Trieste that his first volume of the "Trieste Moon Atlas" was dated (Nov. 15, 1897). His idea was to complete the Atlas in eight volumes, and he estimated the time it would occupy at from eight to ten years. He had so far completed his design that the whole of the telescopic work was finished, and he was engaged on the letterpress description when death overtook him. The first volume of the Atlas, in which there was every promise of a splendid contribution to the knowledge of the lunar surface, consisted of 28 beautifully executed plates, which delineated in all 63 lunar formations. In comparing the plates in

Herr Krieger's Atlas with the actual lunar photographs, one was struck by the excellent results he had obtained.

Mr. C. T. Whitmell read a paper on "Venus at Maximum Brilliancy." He proposed, he said, to consider what variations in maximum brilliancy might occur when account was taken of the eccentricities of the orbits. The result of his investigations was that the ratio of maximum brilliancies in the most and least favourable conditions was $=\frac{120}{100}=\frac{6}{5}$. As Venus and the Earth returned in about eight years to positions nearly identical with their previous positions, they had an approach to a recurring cycle, the conditions in January and March 1902 being practically a repetition of those in January and March 1894.

Mr. Whitmell also read a note on the "Red Flash at Sunset." He said it occurred to him some time ago that, under suitable conditions, there might be seen at sunrise and sunset not only a green flash, but also a red flash, the former at the top, the latter at the base of the Sun's disc, provided that in each case the rest of the disc was hidden, so that glare was absent. In the belfry of the Wesleyan Chapel on the west side of Woodhouse Moor, Leeds, there were narrow horizontal openings, through which, at certain times, the setting Sun could send his rays to an observer at the large band-stand, about 300 yards away. On Saturday, April 26, between 7.15 P.M. and 7.30 P.M., he observed through the openings no fewer than three green and three red flashes. The red ones were seen just as the base of the Sun successively revealed itself below each of the upper edges of the three openings. The green ones were seen just as the top of the Sun disappeared behind each of the lower edges of the openings. Moving aside afterwards, in order to watch the actual sunset on the true horizon, distant some three miles, he observed a beautiful bluish-green flash just as the Sun's tip sunk out of sight at $7^h\ 23^m$.

Dr. Downing said that, with regard to Mr. Whitmell's remarks about the orbits of Venus and the Earth, there was one matter he would like to point out, namely, that the present state of things was not permanent. Nothing in Nature was permanent. The perihelia of the Earth and of Venus were in motion at different rates, and the time would undoubtedly come when the perihelion of the Earth would have the same longitude as the perihelion of Venus.

Mr. A. C. D. Crommelin remarked that Mr. Marth had a rather amusing paper in the *Monthly Notices* several years ago on the "Maximum Brightness of Venus." The *Nautical Almanac* used to predict the time of maximum brilliancy to the minute. Mr. Marth pointed out in his paper that in several instances when the *Nautical Almanac* professed to give the nearest minute it was two days wrong. The reason was that they calculated the distance of Venus from the Earth at such maxima on the assumption of circular orbits.

Mr. G. F. Chambers said he had been in communication with

Señor Iniques, the Director of the Madrid Observatory, as to the suitability of the Balearic Isles as a station for observing the total eclipse of the Sun for 1905. It did not appear that the Balearic Isles offered any advantages over the Spanish mainland. Señor Iniques informed him that the circumstances of the eclipse of April 17, 1912, had already been completely calculated at Madrid. The eclipse, taken as a whole, was an annular one, but would be total for the short time of $4^{s}{\cdot}6$ over the N.W. corner of Portugal and Spain. The central line entered Portugal near Figueira da Foz, and passing near Lamego, Chaves, Bragança in Portugal, and Oviedo in Spain, quitted the land near the town of Gijon.

Mr. Crommelin said he was very much interested in what Mr. Chambers had said about the Spanish eclipse of the Sun of 1912. The results confirmed those which he had got out, viz. that there would be a few seconds of totality in Portugal. Some people claimed that the eclipse would only be annular. It would form a unique phenomenon, having the Sun obscured just for three or four seconds, and no doubt the ring of the chromosphere would be visible round the Sun. He thought also that the spectrum of the "flash" might be photographed right round the Sun's limb.

In reply to Mr. Whitmell, *Mr. Crommelin* stated that he used the value $15'\ 31''{\cdot}76$ for the Moon's mean semidiameter. That was the value used by the American Ephemeris up to the year 1902. The *Nautical Almanac* for 1905, which had recently appeared, used a smaller value for eclipses, viz. $15'\ 31''{\cdot}47$. Even this smaller value would, he thought, give a second or two of totality in Portugal in 1912. Personally he was of opinion that the new *Nautical Almanac* value was too small. It was, however, a great improvement on the one used hitherto, viz. $15'\ 32''{\cdot}65$.

Mr. Chambers said he did not think the Director had expressly stated what value of the Moon's diameter he had used; but he did say something to the effect that the computations had been carried out in a very comprehensive manner.

Mr. George C. Bompas read a lengthy paper on the "Zodiacal Light." The writer reviewed the accounts of the principal writers on the subject and concluded that the meteoric theory of its origin was now generally accepted. The observations he had cited of the extension of the zodiacal light beyond the Earth's orbit in the direction of the Sun's antiapex seemed clearly to indicate elliptical meteor-orbits having their aphelia in the direction beyond the Earth's orbit and their perihelia within it. Such a meteor-system appeared best to explain the phenomena of the zodiacal light. If the zodiacal light was due to the Sun's light reflected from the meteor-streams circling round the Sun, a similar, but brighter, light should be seen in a total eclipse close to the Sun extending along the ecliptic. Such an appearance was presented by the coronal streamers.

Mr. Maunder said he feared Mr. Bompas's very striking and industrious research was only another proof of how difficult it was to form a satisfactory explanation of the zodiacal light. It seemed to be obvious that if it was due to a meteor-stream the aphelion of which was outside the Earth's orbit, its appearance would be very different to that which it now presented, because twice every year the Earth would have passed through the stream. As it approached the stream the breadth of the zodiacal light would have increased rapidly, and probably as the Earth passed through it a meteor-shower would result at a point the co-ordinates of which could be precisely determined. Seeing that the richest meteor-stream of which they knew, namely the Leonids, had utterly failed as yet to be seen out in the open sky, it would seem clear that the zodiacal meteor-stream should show itself on these occasions immensely more magnificent than the November meteor-showers of 1833 or 1866. Further, the general appearance of the zodiacal light would certainly alter as we drew near to it. It would broaden rapidly and its apparent shape and position would change. Nor could he agree that the corona might be the inmost portion of the zodiacal light. The structure was altogether different and the coronal streamers lay in altogether a different direction from the ecliptic, especially in the eclipse of 1898.

Replying to Mr. Whitmell, *Mr. Maunder* said he thought it was generally accepted that the zodiacal light was due to reflected sunlight.

Major L. A. Eddie, of Naawpoort, Cape Colony, contributed a paper on "The Great Southern Comet of 1901," in which he drew special attention to a very faint northern lateral extension from the head, an observation which had been confirmed by Mr. R. T. Innes.

The reading of an interim report of the Variable Star Section contributed by the Director, *Col. E. E. Markwick*, concluded the Meeting.

ROYAL METEOROLOGICAL SOCIETY.

The first of the afternoon Meetings of the Session was held at the Society's rooms, 70 Victoria Street, Westminster, on Wednesday May 21, *Mr. W. H. Dines, B.A.*, President, in the Chair.

Capt. D. Wilson-Barker read a Report, prepared by *Mr. Dines* and himself, on the Wind-force Experiments which had been made on H.M.S. 'Worcester' off Greenhithe, and at Stoneness Lighthouse, 817 yards from the ship, on the north bank of the river. These experiments were in continuation of those on the exposure of anemometers at different elevations which were carried out on the 'Worcester' a few years ago. All the observations were made with the pressure-tube anemometer. The broad general result is that the lighthouse experiences steadier and stronger winds than the 'Worcester,' the velocity being about 6 per cent. greater, notwithstanding the fact that the elevation is less than one half; but that in both positions the extreme velocities reached in the gusts are about equal.

Dr. H. R. Mill read a paper on "The Cornish Dust-fall of January 1902." When the West of England newspapers of January 24th announced falls of "pink snow" and "muddy rain" in several parts of Cornwall and South Wales, it seemed to the author possible that fresh light might be thrown on what is, at present, the chief object of progressive meteorology, viz., the movements of the upper air. He therefore took steps to collect as much information as possible from the whole of the district, and found that the phenomenon was reported from 75 different places in the south-west of England and Wales. These were all south of a line joining Milford Haven and Chepstow, and west of the meridian of Bath. By means of a map Dr. Mill showed that four separate areas were visited by the dust between January 21 and 23, viz.:—(1) Cornwall, 1400 square miles; (2) North Devon, 150 square miles; (3) Milford Haven, 50 square miles; and (4) Bristol Channel, 600 square miles. The dust appears to have been confined mostly to low rather than high ground, for none was reported to have fallen on the Mendip Hills, Dartmoor, Exmoor, and the Welsh mountains. The observations show that the 22nd was undoubtedly the day when most falls occurred, and that the colour of the dust was yellowish or brownish. From a consideration of the meteorological conditions at the time and for several days before, the author is inclined to believe that the evidence points to the dust having been transported in the upper air from the African deserts.

Real Paths of Eighteen Meteors observed at Two or more Stations during the Years 1875–80.

Included in some of the last reports of the Luminous Meteor Committee of the British Association are records of certain accordances discovered among meteor observations, the discussion of which was postponed to a more convenient season. Mr. Denning has recently pointed out to the writer the desirability of computing real paths in these cases, and the results obtained are tabulated below. Two other meteor-paths have been added, namely, for the objects of 1877, May 30, $11^h\ 26^m$, and 1879, November 12, $10^h\ 18\frac{1}{2}^m$, also mentioned by Mr. Denning as requiring calculation. The latter of these was a very interesting Leonid, described at Bristol as appearing (with a path 98° long) when the radiant was just rising; the former was an early member of a shower that becomes very conspicuous during August. The resulting beginning-heights of two meteors, as well as the real velocities of a few, are so large that it is probable that such a great divergence from the usual values originates in what may perhaps be the approximate nature of some of the observations. The most important among the remaining meteors are an early and a late Perseid and a Quadrantid.

Date.	G.M.T.	Mag.	Height at Beginning.	Height at Ending.	Length of Obs. Path.	Velocity (per second).	Began over.	Ended over.	Radiant-point.		Name of Meteor.	Observers.
1875.	h m		miles.	miles.	miles.	miles.			α °	δ °		
Nov. 14	12 53½	2->1	92	76	31	...	Willingham, Lincs.	2 miles W. Shirebrook.	154	+37	μ Ursid.	W. C. N., S. J. P.
1876.												
July 19	10 58	1–♃	71	47	42	Rather swift.	Highgate.	2 miles S. Godalming.	22	+54	Perseid.	W. F. D., J. L.
20	11 28½	1	86	32	110	...	10 miles N.E. St. Omer, France.	10 miles S.W. Eastbourne.	351	+25	78 Pegasid.	W. F. D., J. L.
21	10 26	1–♀	65	44	43	71	6 miles N.W. Romsey.	5 miles N.W. Calne.	294	– 9	κ Aquilid.	W. F. D., J. L.
24	11 35½	1–♃	140(?)	45	94	63	4 miles E. Hornsea.	13 miles E. Hornsea.	292	+51	θ Cygnid.	W. F. D., J. L.
25	10 55	1	69	36	34	...	22 miles S. Bridport.	12 miles S. Bridport.	280	+36	α Lyrid.	J. E. C.*, W. F. D.
Aug. 10	9 57	♃–2×♀	76	54	58	...	8 miles N. Wellington, Salop.	6 miles N.E. Builth.	42	+50	Perseid.	T. W. B., H. C., W. F. D., T. H. W.
11	10 27	1	107	42	87	Rapid.	13 miles S.E. Worthing.	52 miles S. Havant.	355	+47	ι Andromedid.	W. F. D., J. L.
14	11 4½	3–2	67	48	33	66	3 miles S.W. Llanrhaidr.	5 miles E. Harlech.	3	+15	γ Pegasid.	W. F. D., J. L.
14	11 24	3	28	25	10	...	3 miles E. Ross.	6 miles N. Monmouth.	18	+12	ε Piscid.	W. F. D., J. L.

1877.												
May 30	11 26	♃	96	63	74	37	88 miles E. Cromer.	116 miles E. Ipswich.	20	+58	δ Cassiopeid.	H. C., W. F. D.
Aug. 7	11 44	3	46	35	27	Rapid.	4 miles W. Newport, Essex.	Brentwood.	95	+62	δ Aurigid.	W. F. D., J. L.
10	11 5	4–2	88	72	17	45	12 miles N.W. Barnard Castle.	2 miles S. Middleton.	284	+67	δ Draconid.	J. E. C.†, W. H. W.
1878.												
Oct. 22	12 25	5–2	78	71	44	...	5 miles E. New Romney.	2 miles W. Gravesend.	98	−12	α Canis Majorid.	H. C., W. F. D.
22	12 45	2	76	58	35	...	40 miles S.E. Great Yarmouth.	10 miles E. Hopton, Norfolk.	86	+ 6	α Orionid.	H. C., W. F. D.
1879.												
Nov. 12	10 18½	4–3	76	69	264	59	8 miles N.E. Tonbridge.	94 miles S. Looe.	151	+25	Leonid.	H. C., W. F. D., T. P. G.
1880.												
Jan. 2	7 50	♃	77	62	57	Slow.	5 miles S. Spilsby.	8 miles N.E. Cambridge.	232	+52	Quadrantid.	H. C., W. F. D.
Oct. 29	9 50	>♃–>♀	158(?)	73	134	89	45 miles S. Beachy Head.	4 miles N.W. Ringwood.	46	+15	ζ Arietid.	J. B., W. F. D.

The observers, with their several stations, were as follows:—

T. W. Backhouse, Sunderland.
J. Baxendell, Southport.
H. Corder, Chelmsford.
J. E. Clark, (*), Street, Somerset.
„ (†), York.
W. F. Denning, Bristol.
T. P. Gray, Bedford.
J. Lucas, Oxford.
W. C. Nash, Greenwich.
S. J. Perry (Rev.), Blackburn.
T. H. Waller, Birmingham.
W. H. Wood, Birmingham.

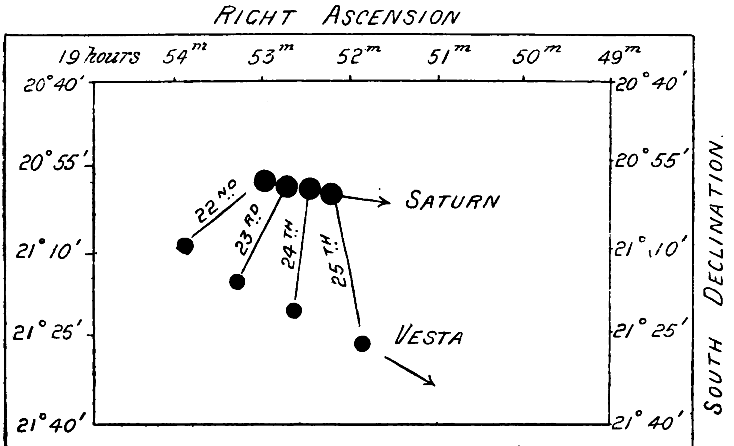
WALTER E. BESLEY,

75 The Chase, Clapham Common,
London, S.W. 1902, May 5.

Conjunction of Saturn with Vesta.

An excellent opportunity will be afforded for picking up Vesta on about June 24, when the planet passes within half a degree south of Saturn. The relative positions of the two objects at transit on several nights near the time of their nearest approach will be as follows:—

1902.	Saturn.		Vesta.		Meridian passage.
	R.A.	Dec.	R.A.	Dec.	
	h m s	° ′ ″	h m s	° ′ ″	h m
June 22...	19 53 2	−20 57 15	19 53 57	−21 8 5	13 52
23...	19 52 46	−20 58 2	19 53 17	−21 14 15	13 48
24...	19 52 30	−20 58 51	19 52 36	−21 20 30	13 43
25...	19 52 14	−20 59 39	19 51 53	−21 26 50	13 39



Conjunction of Saturn and Vesta
1902 June 24.

Vesta will be about 6th mag. at the time; but the Moon, being only a few days past the full, the planet will be invisible to the naked eye. Both Vesta and Saturn may be seen in the same telescopic field, the distances separating the objects being about 17′ on June 22 and 23, 21′ on June 24, and 26′ on June 25. There will be no bright stars near. The two planets will rise about 9.30 P.M.

Bishopston, Bristol,
1902, May 18.

W. F. DENNING.

The Flash-spectrum.

THE remarks made by Mr. Evershed and Mr. Mitchell in the last number of the *Observatory* suggest that the full significance of enhanced lines has not yet been completely grasped by them. The enhanced lines in the spectrum of a metal are conveniently defined as lines which are brighter in the spark than in the arc spectrum; but the spectrum of α Cygni, which consists almost entirely of enhanced lines, indicates that they are much more than this, namely, that under certain conditions they constitute a separate spectrum, in the case of several metals, quite free from admixture with the ordinary arc lines, and should therefore be discussed almost as if they were spectra of new substances.

Mr. Evershed has certainly got so far as to recognize that seven abnormally bright lines of the flash-spectrum coincide with enhanced lines of iron as given by Lockyer. Other coincidences, however, he thinks are of no significance, because the solar lines in about the same position have a double or multiple origin; but, admitting the undoubted presence of other enhanced lines of iron, it must be allowed that a coincident enhanced line, although in a group, contributes its share to the intensity of the corresponding flash-line and must not be regarded as absent. The weaker enhanced lines, like the weaker lines of any other spectrum, may, of course, be wanting. Mr. Evershed states that 14 of the 21 coincident enhanced lines of iron which he finds are of "normal intensity" in the dark-line spectrum; I do not know the exact lines to which he refers, but if any of them are included in the following table, I fear I should not be in agreement with him. In this table I have selected the solar lines of intensities 3, 4, and 5 given in Rowland's tables for a very limited part of the spectrum, which are attributed to iron, or which have since been admitted to be due to iron, and have compared them with the flash-lines tabulated by Mitchell, the lines throughout being so selected that there is no danger of confusion with lines of other elements.

Selected Iron Lines in Sun and Flash.

λ Rowland.	Intensity in Sun.	λ Flash, Mitchell.	Intensity in Flash.	? Enhanced Line.
4484·392	4	4484·4	0	No.
85·846	3	......	...	No.
89·911	4	......	...	No.
4508·455	4	4508·5	3	Yes.
15·508	3	15·6	2	Yes.
17·702	3	17·7	1	No.
20·397	3	20·3	2	Yes.
25·314	5	25·0	0	No.
31·327	5	31·4	1	No.
48·024	3	......	...	No.
84·018	4	84·0	4	Yes.
98·303	3	98·3	0	No.
4602·183	3	4602·1	1	No.

The obvious inference is that the *normal* intensity in the flash-spectrum of an ordinary solar line of intensity 3, 4, or 5 due to iron is something less than 1, while the average intensity for the four enhanced lines in the flash reaches the *abnormal* amount of 2·75 on Mitchell's scale. Surely, we have here an indication that the enhanced lines of iron are of special significance in the spectrum of the flash-stratum.

Mr. Mitchell states that in the flash-spectrum he certainly finds 30 out of the 53 enhanced lines of titanium for which wave-lengths have been published by Sir Norman Lockyer, but adds that "every one of these 30 lines, without exception, appears as a strong line in the solar spectrum," and goes on to say that the coincidences prove nothing, because "where there are strong Fraunhofer lines we expect strong reversals in the flash." The following table, in which I give solar lines of intensity 3, which Mr. Mitchell agrees may be safely attributed to titanium, will indicate what is here understood by a "strong" Fraunhofer line:—

Selected Titanium Lines in Sun and Flash.

λ Rowland.	Intensity in Sun.	λ Flash, Mitchell.	Intensity in Flash.	? Enhanced Line.
4078·631	3	......	...	No.
4300·211	3	4300·4	3	Yes.
4313·034	3	4312·9	3	Yes.
4417·884	3	4418·0	3	Yes.
4512·906	3	......	...	No.
4518·198	3	4518·2	1	No.
4527·490	3	4527·5	1	No.
4544·864	3	4544·9	0	No.
4590·126	3	4590·1	2	Yes.

Seeing that four of the enhanced lines occur in this table, we may take it that Mr. Mitchell considers a line of intensity 3 in the Sun to be a "strong" line; if this be so, I would ask why *all* the above lines do not appear as strong lines in the flash, as he says he would expect them to do, and why the enhanced lines, as in the case of iron, are not only present in the flash, but are among the strongest lines?

Differences in the heights to which different vapours extend may perhaps be a factor in determining the relative brightnesses of lines of different elements in the flash, but cannot be the sole explanation of inversions of intensity in lines belonging to the same element.

A perusal of Mr. Mitchell's complete tables confirms the ideas which I formed from his abstract. It is only by a process of averaging the intensities of the Fraunhofer and flash lines that he could be led to make such a statement as that "for each and every element the brighter the solar line the brighter the flash-line corresponding to it." Of the very last two lines which he gives, each of which is undoubtedly due to iron, the one at 4924·1 is of intensity 5 in the Sun and 3 in the flash, while that at 4920·7 is intensity 10 in the Sun and only 1 in the flash. Need it be added that the former is an enhanced line and that the latter is not?

The facts above stated, together with the more complete discussion given in Sir Norman Lockyer's report on the eclipse of 1898, indicate a very close connection between the flash-spectrum and the enhanced lines, and appear to show that the flash-spectrum is not a mere reversal of the Fraunhofer lines, either as a whole or with variations from element to element. We seem, in fact, to be driven to the conclusion that while the Fraunhofer spectrum closely approximates to the arc-spectrum of the various metals represented, the flash-spectrum is produced, at least in great part, by vapours in a condition intermediate between that of the arc and that which gives the enhanced lines alone in such a star as *α* Cygni. The latter spectrum confirms the results of experimental work in showing that the purely enhanced-line spectra consist of comparatively few lines, but they are not the less important for that; no one doubts the presence of sodium in the Sun, although there are only 11 coincident lines as against the 2000 which demonstrate the presence of iron. The passage from the complex arc-spectrum to the comparatively simple enhanced-line spectrum is not abrupt, and it is for this reason that in a vapour of intermediate state lines of both descriptions are found. In the case of stellar spectra the gradual change from one spectrum to the other can be easily traced.

The precise difference between arc and enhanced lines may not be perfectly understood, but if we attribute it to the higher temperature of the spark, in accordance with the general view, it does not necessarily lead to any improbable suppositions as to the distribution of temperature in the neighbourhood of the photosphere. Sir Norman Lockyer's opinion that the Fraunhofer absorption is produced by the absorption of cool vapours above the flash-stratum involves a distribution of temperature which presents no difficulty. But if the Fraunhofer absorption is produced by vapours which are beneath the flash-stratum, it would seem that we have a hotter stratum overlying a cooler one, which would at first sight seem impossible. On this point, however, I may quote the following remarks made by Dr. Johnstone Stoney in 1896 (*Journ. B. A. A.* vol. vii. p. 103):—"Everywhere inside this photospheric layer, and for a short distance outside, there prevails a higher temperature, so that the photosphere is a layer of minimum temperature with higher temperatures on either side of it."

Royal College of Science,
South Kensington.

A. FOWLER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Cassini and the Solar Parallax.

GENTLEMEN,—

There have been many martyrs of science besides those recorded in Brewster's well-known book; and amongst these the Abbé Chappe d'Auteroche was undoubtedly one. Born in Auvergne in the year 1722, he made an expedition into Siberia to observe the transit of Venus in 1761, and successfully accomplished his object, taking observations of both phases of the phenomenon at Tobolsk. His account of his journey was published at Paris in 1763, and as some parts of it were far from complimentary to the Russian government of that country, a work called "L'Antidote ou Examen du mauvais livre intitulé 'Voyage en Sibérie fait en 1761, contenant les mœurs, les usages des Russes et l'état actuel de cette Puissance,'" appeared at St. Petersburg seven years afterwards, and was attributed to the Empress Catherine II.* The same devotion to science which led Chappe to brave the perils of a Siberian journey in those days induced him eight years later to run what proved to be a greater risk, in crossing the Atlantic Ocean, and proceeding through Mexico to California, for the purpose of observing the transit of 1769. From this journey he never returned. The whole party were in turn smitten with fever not long after the phenomenon had taken place; tenderly did

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* There is an English translation by "An Englishwoman" (1772) and dedicated to the Czarina.

Chappe care for the others till seized himself. It is thought that he might have recovered but for his temerity in observing whilst apparently convalescent the eclipse of the Moon on June 18. He died on August 1; and his papers were brought home by Pauly, almost the only survivor of the party. The transit of Venus was observed at San Joseph, not far from Cape San Lucas, the southern point of the peninsula of California, not the most favourable position, but Chappe feared time would not allow (the voyage and journey having taken longer than he expected) of proceeding further.

His diary and observations were put into the hands of J. D. Cassini, son of the director of the Paris Observatory (and then styled "directeur en survivance"), who afterwards became, on his father's death, director, taking the title of Comte de Thury. He published the account of Chappe's journey in his own words, with a discussion of the results of his observations—the work appearing in 1772 under the title "Voyage en Californie pour l'observation du passage de Vénus sur le disque du Soleil, le 3 Juin, 1769." We are all familiar with Stone's remarks on the observations of Chappe and Pauly in the *Monthly Notices*, vol. xxviii. p. 255, soon after his discussion of the observations of Mars at its opposition in 1862 had made it clear that Encke's value of the solar parallax was about a quarter of a second too small.

Cassini appends to his work on Chappe's journey and observations a supplement entitled 'Histoire abrégée de la Parallaxe du Soleil,' in which he gives a sketch both of previous attempts to determine the solar parallax, and a brief examination of the observations of the transits of Venus in 1761 and 1769 with their results. His final conclusion is: "la parallaxe moyenne de huit secondes et demie étant celle qui s'accorde avec les meilleures observations, celle qui avoit déjà été indiquée par le passage de 1761; on peut, à ce que je crois, l'adopter, sans craindre de s'éloigner beaucoup de la verité." But as most of the values which he quotes exceed this by about 0″·2, I presume he means that he did not consider the parallax known more accurately than to the nearest half-second. Lalande, rejecting the Wardhus observations, found 8″·5; Pingré's determination amounted to 8″·88, 0″·08 greater than what is now internationally accepted as the most probable true value, 8″·80. It was very unfortunate that Cook's party was the only one which succeeded in observing the transit in the southern hemisphere. Chappe had originally intended to endeavour to do so, and to observe the transit on one of the Solomon Islands, but he could only have reached that region on a Spanish vessel and by the consent of the court of Spain, which was very unwilling that foreigners should acquire a knowledge of those seas, and therefore proposed to him as an alternative to afford him a passage to Mexico for observation in California, which was what he actually carried out. Véron also hoped to observe in the southern hemisphere, and for that purpose accompanied Bougainville in his voyage round the world. The ship got out of the southern seas too soon, and he arrived at Pondicherry (where he soon afterwards died) too late for the transit.

Yours faithfully,
Blackheath, 1902, May 1. W. T. Lynn.

The Solar Eclipse of August 28, A.D. 360.

Gentlemen,—

Mr. Monck, in your last number, calls attention to this eclipse, which he says seems to him "to prove that the tables used in the computation were inaccurate." The eclipse in question is certainly one of the best attested in history, being mentioned by a trustworthy historian, who was himself an eye-witness. Its circumstances are discussed by Prof. Ginzel in his monumental work 'Spezieller Kanon der Sonnen- und Mondfinsternisse,' p. 212. He thinks it quite possible, according to our present tables, that a small eclipse (somewhat augmented by refraction) may have been visible at sunrise to the Roman army in the position it occupied, and that reports (perhaps derived from Persian prisoners) were afterwards received of its having been much larger and of longer duration in regions further to the east, which led Ammianus to modify in his history the account of what he had himself seen. Indeed as it stands it cannot apply to an eclipse as witnessed at one place. Ginzel concludes with the remark:—"Das für den gemeinen Mann Wunderbare an der Sache mag Ammianus veranlassen, von dem Hervortreten von Sternen 'vom frühen Morgen bis zum Mittag hin' zu sprechen."

Yours faithfully,
Blackheath, 1902, May 5. W. T. Lynn.

Photographic Surveying.

Gentlemen,—

The refreshing discussion at the meeting of the Royal Astronomical Society, reported in the April number of the *Observatory*, calls for a word from me. Prof. Turner and I need scarce differ about the priority of the idea of determining the position of points from their pictures on a pair of plates exposed in the same vertical plane and at the ends of a base, for the theory of the method is practically the same with that of stereoscopic pictures given in text-books of Geometrical Optics, Heath's for instance, the sole difference being one of scale. Beyond this, our common ground, there only remain the interesting developments which Prof. Turner has given, and the idea of the application of stereoscopic measurement to surveying, which Prof. Turner says had not occurred to him. My object in calling attention to my previous paper on the subject was simply to be allowed to bring the work I had undertaken to a conclusion.

A grant has been made by the Prime Minister of the Cape Colony for the construction of the apparatus proposed by me, so that I hope the complete method will in due course be submitted to a practical test, which may possibly show that there is something more in the views of Sir David Gill, Prof. Turner, and myself than Major Hills is willing to allow. Meanwhile, I send you a stereoscopic print from portions of two negatives exposed by me early last year, which indicates sufficiently the advantages that stereoscopic examination of plates affords in the automatic identification of all corresponding pairs of points and in bringing into prominence topographical features not obvious in either picture.

For want of suitable measuring-instruments, the chief use I have made of the stereoscopic method, occasionally since 1898, has been in the sketching in of topographical features between points determined by ordinary survey. In this simple form the method has given me remarkable help in analysing badly defined topography, and that led me to attempt to perfect it.

Yours faithfully,
H. G. Fourcade.

Forest Department, Cape Town,
1902, May 6.

NOTES.

Comet Notes.—No observations of Brooks's Comet (*a* 1902) later than April 19 are reported. Had Mr. Brooks not discovered the comet on April 15, it would probably have escaped detection altogether, as it quickly passed to a region unfavourable for observation. Prof. Wolf obtained a photograph of the comet with 7 min. exposure on April 18. It was very faint and scarcely measurable; there was no perceptible nucleus, but a broad fan-shaped tail in position-angle 300°. The southern concave edge of the tail was distinctly brighter than the northern (*Astr. Nach.* 3792).

Popular Astronomy for May contains a search ephemeris for Comet 1895 II. (Swift), by Mr. F. E. Seagrave, based on Mr. H. R. Morgan's elements in *Astr. Journ.* 451. The comet approaches Jupiter's orbit within about 5 millions of miles in longitude 179°; but no very close approach has occurred since the last apparition, so that the perturbations (which have not been computed) are probably not large.

G.M.T.	R.A.	S. Dec.	G.M.T.	R.A.	S. Dec.
d h	h m s	° ′	d h	h m s	° ′
1902 Sept. 3 5...	16 15 56	17 55	1902 Nov. 3 5...	19 16 52	21 21
10 14...	16 31 10	18 46	9 14...	19 41 16	20 46
17 18...	16 47 55	19 35	15 23...	20 6 3	19 56
24 18...	17 6 1	20 18	22 8...	20 31 2	18 51
Oct. 1 14...	17 25 21	20 55	28 19...	20 56 7	17 32
8 7...	17 45 51	21 23	Dec. 5 7...	21 21 10	15 59
14 21...	18 7 21	21 41	11 21...	21 46 4	14 13
21 9...	18 29 47	21 48	18 14...	22 10 44	12 16
27 20...	18 52 59	21 42	25 10...	22 35 6	10 10

Perihelion passage Nov. $9^{d}\cdot6$; comet brightest Oct. 27, when it is distant 1·29 from the Earth, 1·31 from the Sun.

‘Publications of the Lick Observatory,’ vol. vii. part 1, contains a “Short Method of Determining Orbits from Three Observations,” by A. O. Leuschner. The three observations are supposed to be so near together that the R.A. and Dec. can each be expressed in the form $A+Bt+Ct^2$, the higher powers being neglected. Transformation to the ecliptic is deferred till the final elements are deduced, this saving much labour and risk of error. In these respects the method is identical with that given by Airy in the ‘Memoirs of the Royal Astronomical Society’ some sixty years ago: but some improvements are introduced here, notably a utilization of Table xiii. *a* in Oppolzer's Bahnbestimmung to more rapidly solve the equation of the 7th degree in z (the ratio of the distance of the comet to that of the Sun).

Elegant methods are also indicated of obtaining a closer approximation, and of introducing the corrections for parallax and aberration without repeating the whole work *ab initio.*

Two fully worked-out examples are given, viz., Comet 1900 III. and Planet 1900 G.A. The observations used for the comet were made by Mr. Aitken on 1900, Dec. 24, 26, 28; the photographs of the planet were taken on 1900, June 28, 30, July 2. The method seems a very convenient one both for obtaining first approximations to an orbit, and afterwards for correcting them to represent later observations. A. C. D. C.

MINOR PLANET NOTES.—Three new planets were discovered at Heidelberg on May 7: HX, HY (each of mag. 12·5) by Wolf, HZ (mag. 12·0) by Carnera.

THE CONSTANT OF ABERRATION.—The evidence that the adopted value of this quantity needs reconsideration is still accumulating. From 1857 until the end of the last century, W. Struve's value 20″·445, found from observations with the prime-vertical transit instrument at Poulkova, was used in the computation for the *Nautical Almanac*, and in the almanac for the current year 20″·47 is given as the adopted constant, but many determinations which take account of the variation of latitude tend to show that the value is well above 20″·5. A recent number of the *Astronomical Journal* (No. 520) contains two investigations by Dr. Chandler: one, a discussion of observations made by Talcott's method at various latitude-stations, gives 20″·555 as the value of the constant of aberration; the other, which is a re-examination of Pond's observations of Polaris with the Troughton mural circle, gives 20″·578 as the value of the same quantity. Dr. Chandler adds that the general mean obtained from an unpublished discussion of all the extant determinations of the aberration constant suitable for use (more than forty in number) shows the true value of the constant to be very close to 20″·53. Incidentally the second of these two investigations shows the parallax of Polaris to be −0″·006, and the adoption of the new value for the aberration constant will explain certain discordances found by Dr. Auwers in his reduction of these observations, for which he applied empirical corrections.

THE PARALLAX OF NOVA PERSEI.—Dr. Chase, of Yale College Observatory, publishes in *Ast. Journal,* No. 520, a value of the parallax of the Nova in Perseus deduced from observations with the heliometer between 1901 February 24 to 1902 February 9. The distances of the object were measured from two comparison stars of about the eighth magnitude, the light of the Nova being screened down to about this magnitude in the earlier part of its career, and the parallax relative to these stars comes out −0″·012, with a probable error of ±0″·014; so that if, as Kapteyn suggests, the average parallax for an eighth magnitude star is +0″·007, the absolute parallax of Nova Persei would be −0″·005. A somewhat similar determination by Dr. Ernst Hartwig, of Bamberg, shows the parallax to be negative—that is to say, it is farther away than the comparison stars.

THE LEEDS ASTRONOMICAL SOCIETY.—We have before mentioned the Journal of this enterprising Society as supplying most interesting reading, and the volume for the year 1901, which we have lately been favoured with, in no way belies its reputation. Besides a succinct account of the astronomical events of the year, Nova Persei, the Comet, the Conjunction of Jupiter and Saturn, and other events, it contains several articles on diverse subjects which are all very thought-inspiring. Mr. Ivo Gregg contributes a valuable article on Meteors; Mr. Watson has another on the Twelve Movements of the Earth, which, while it does not bring out any absolutely new facts, is an excellent summary of the geometry of our planet. A philosophical study of the Relation of Man to the Universe, by Mr. W. D. Barbour, and another of Mr. Whitmell's ingenious investigations, "The Planet Neptune as a View Point," find place in this issue of the journal, with other things which space prevents us from mentioning in detail.

THE RED SPOT ON JUPITER.—Mr. Stanley Williams' observations of the Red Spot, though made under unfavourable conditions owing to the great S. declination of the planet, shows (*Ast. Nach.* No. 3786) that the rotation-period of this object is shortening, a fact which has been noted by other observers. The mean rotation-period of the spot in 1901 was found to be $9^h\ 55^m\ 40^s{\cdot}92$, which is $1^s{\cdot}38$ less than was shown by Mr. Williams' observations in the previous year. This observer confirms Prof. G. W. Hough's conclusion that there has not been any material change in the length of the spot during the past twenty years.

SATURN VISIBLE THROUGH THE CASSINI DIVISION.—Mr. Whitmell points out (*Ast. Nach.* 3793) that on July 17 next an observation will be possible of a kind of which there are only a few records*. Viewed from Saturn, the Sun and the Earth will have the same elevation above the plane of the ring, so that the streak of sunlight which passes through the Cassini division on to the ball of the planet will be reflected back to certain parts of the Earth through the same division, or, in other words, it will be possible to see from some places (but the exact position of these is not easily calculable) Saturn through the division in the rings. On the same date a Saturnian observer will be able to see a solar transit of the Earth.

VOLCANIC ERUPTIONS IN THE WEST INDIES.—During the past month the world has been startled by a catastrophe of Nature probably unparalleled. Two slumbering volcanoes in islands of the West Indian group have suddenly burst forth, with most disastrous effects to the inhabitants of the towns near. This would scarcely call for mention in these pages but for the fact that astronomical causes have been assigned for the eruption. In the first place, the first eruption happened at time of new Moon, and when the Moon, which was at an unusually close perigee, was nearly in the zenith of the volcano; and as it has been suggested that Earth-tides caused by lunar and solar attraction may cause earthquakes, it is pointed out that the circumstances favoured this hypothesis. Sir Norman Lockyer seeks to show a connection between eruptions such as this and the sun-spot period, a connection which he says was suggested by Wolf half a century ago. He states that in 1867, which was a time of minimum, there were volcanic eruptions in South America, Formosa, and of Vesuvius. At the maximum of 1871–72 there were eruptions at Martinique and St. Vincent. At the maximum of 1883 the Krakatoa explosion happened; and this year it is a time of minimum. But it is difficult to see why both great and small solar activity should cause these things.

---

* In the number (vol. xii. 7) of the B. A. A. Journal just published, Mr. Whitmell states that Prof. C. A. Young, to whom he had communicated his note, was able to give him previous instances. The references happen to be in vol. vii. of this Magazine. Mr. Lassell saw the phenomenon in 1852, and Prof. Young and Prof. A. Hall in 1883.

STONYHURST COLLEGE.—It seems scarcely necessary to inform our readers of the contents of the Annual Report of this institution so far as they relate to astronomy, for Father Sidgreaves communicates his results so promptly to the Astronomical Society that their substance has already appeared in our pages. It will be seen from another page in this number that the spectroscopic observations of Nova Persei formed a large part of the work for last year. During the interval in the summer months, when the Nova was not well placed for observation, the prismatic camera was employed on Arcturus and α Cygni for the construction of its wave-length curve, and β Lyræ and γ Cassiopeiæ for the extension of their spectra into the shorter wave-lengths.

It is recorded that drawings of sun-spots were made on 235 days, which is not strictly true, for though this is the number of days on which the solar surface was examined, on 91 of them there were no spots to draw; and in connection with this fact it is worth while turning to another page of the booklet which gives the dates of magnetic disturbance. It is seen here that there was no day of great disturbance during the year, and comparatively few to which the word moderate could be applied. The total rainfall during the year was 8 inches below the average, which was a loss of 17 per cent. to the annual water-supply.

ASTRONOMICAL EPHEMERIDES. — The following has been communicated by Prof. A. Hall to the *Astronomical Journal* (No. 521):—

> The managers of the *Connaissance des Temps* have taken a step which might well have been taken twenty years ago; that is, they will omit the lunar distances after 1905. With the great extension of accurate determinations of longitude, by means of telegraphic lines, the need of lunar distances has become small, and they are rarely used. If a case should occur it could be computed directly by some of the simple formulas such as Bremiker's. Such a change in the *American Ephemeris* would save yearly seventy-two pages of printed matter.
>
> Again, what is the use of so much space being given to the small stars occulted by the Moon? Probably one-fifth of this space is sufficient to give all the data really needed for the bright stars and the groups.
>
> Although time will no doubt show defects in the theories of the planets, our present tables appear to be accurate and well established. This is not the case with the secondary systems. The twenty satellites of the outer planets need new orbits and tables, made with a uniform method and notation. It is time that the Saturnian system should be investigated as a whole, and not by parts. The great telescopes can now furnish good observations, and it is to be hoped that observers may unite on one plan. On account of the ring and the figure of the planet, and its brightness, *Titan* is a good centre to which the observations can be referred.
>
> Professor H. Struve has begun a good work on this grand system, and his investigations should be continued. The figure of the planet, the attraction of the ring, the action of the satellites on each other, the curious motions of *Hyperion*, and the action of the Sun on the satellites, ought to be discussed in a general manner. This is a work that might well be undertaken by the Office of the *American Ephemeris*. The theories of *Jupiter's* satellites also need revision and new tables.
>
> In the future, stellar astronomy will demand more attention. Besides the

proper motions of the stars and the motion of the Sun in space, which are being slowly disclosed, it would be well, I think, if the rapid double stars, the variable stars, and stars with peculiar and interesting motions could be noticed in a way that would direct the observers. They would need but a small space in the *Ephemeris.*

Gothen, Conn., 1902, May 7.

The Cold Weather in May.—After a period of exceptional warmth in April, lasting from the 15th to the 27th (the thermometer registering 68°·2 on April 19), very cold and inclement weather set in and continued till May 22. Temperature during this period ranged between 58°·3 on May 17 and 29°·8 on May 14, the mean being 44°·8, which is 6°·7 lower than the average value. In the past 60 years no other period of such extreme severity at the latter part of spring appears to have been recorded; but in 1874 in a similar period of 20 days, from May 1 to 20, the mean temperature was 46°·6; in 1885, for the 22 days May 1 to 22 the mean was 47°·4; and in 1845, for the 23 days May 4 to 26, the mean was 47°·5. Cold rains and hail fell on 18 days to the amount of 2·77 inches, being very nearly the same amount as the rainfall recorded on 37 rainy days in the three months January to March. Until the 23rd sunshine was very deficient, amounting only to 25 per cent. of the possible amount; but after the 24th, when the weather became warm and sunny, the sunshine for the four days May 24 to 27 sprang up to 72 per cent. of the possible duration.

W. C. N.

Obituary.—Announcement is made of the death, at the early age of 37, of Herr J. N. Krieger, an amateur astronomer who had devoted himself since 1890 to a study of the Moon's surface, and to making an Atlas of our satellite. An account of this work by Mr. Goodacre will be found on p. 225 of this number.

Astronomy was represented at the Royal Society Conversazione on May 14 by an exhibition of photographs of the planet Eros, taken at the Cambridge Observatory, and diagrams of results deduced therefrom, which were ably explained by Mr. Hinks. By a series of Spectra of Meteorites, and other elements, mainly terrestrial, exhibited by Sir Norman Lockyer, and by the MSS. relating to the discovery of Neptune, sent by St. John's College, Cambridge, through Prof. Sampson. Photographs of the Nebula surrounding Nova Persei, taken at the Yerkes Observatory, were also on view.

The list of Candidates selected by the Council for election as Fellows of the Royal Society contains the names of two astronomers —Hugh Frank Newall, of the Cambridge Observatory, sometime Secretary of the Royal Astronomical Society, and Sidney S. Hough, Chief Assistant of the Royal Observatory, Cape of Good Hope, to whom we offer our congratulations.

We are informed that Dr. Doberck, the Director of the Hong Kong Observatory, is resigning his post on account of ill-health.

The Observatory of the late Col. Cooper at Markree, Ireland, which was in the charge of Dr. Doberck before he went to Hong Kong, and has since been under the charge of Dr. Marth and Mr. Henkel successively, is to be for the present disused.

Visitation-Day at Greenwich Observatory will be on Saturday, June 7. The Visitors will dine, as in former years, at the Criterion Restaurant, Piccadilly Circus.

The next Meeting of the Royal Astronomical Society will be on Friday, June 13; of the British Astronomical Association on Wednesday, June 25.

From an Oxford Note-Book.

Few questions are of wider importance to the practical astronomer at the present time than that of the optical distortion of object-glasses and mirrors. How faithful are the photographs we are taking? Much has been done to elucidate this point, and the answer is generally in favour of the photographs; but much more remains to be done. An interesting contribution to the literature of the subject has just been made by Mr. Harold Jacoby, of the Columbia College Observatory, who has measured some photographs taken at the Cape Observatory with the Astrographic Telescope. These photographs were specially taken for the investigation of one particular form of possible optical distortion, viz., that which is represented by a difference of scale-value in different directions. Mr. Jacoby had the happy thought that if the same stars were photographed several times, with the telescope rotated round its optical axis, a comparison of the results should show any scale-difference of this kind. Rotation round the optical axis can be conveniently obtained, without any special structural arrangement, by photographing the Pole, with the instrument set to different hour-angles: and in this way accordingly four plates were taken at the Cape, measured and compared at Columbia College; and, as a result, Mr. Jacoby announces that there is no optical distortion of the kind considered.

Unfortunately for his conclusion, his own figures prove exactly the reverse. He has exhibited his residuals in a diagram, from which it is not easy to make any definite inference at a mere glance, but from which, with a little patience, the facts may be gathered. It is easier, however, to take the actual figures. Forming the mean of his residuals of $\Delta\alpha \sin p$ for every 3 hours (*i. e.*, 23^h–1^h, 0^h–2^h, 1^h–3^h, &c.), we get the following results, the unit being $0''\cdot01$: +7, +11, +12, +7, +3, +4, −4, −9, −11, −7, −5, −4, 0, +3, +7, +8, +8, +7, 0, −4, −8, −12,

−9, −7. This represents an inequality with coefficient $0''\cdot11$; which should be increased to $0''\cdot14$, because it has been reduced in the process of taking means for every 3 hours. Now if we have a distortion

$$\Delta y = 2\mu . y, \quad \Delta x = 0$$

(representing a different scale-value in x and y), the corresponding effect on polar co-ordinates (r, θ) is

$$\Delta r = \mu r(1 - \cos 2\theta), \quad r\Delta\theta = \mu r \sin 2\theta.$$

We are concerned with the second of these, and $r = 2000''$ approximately. Hence

$$\mu = 0\cdot14/2000 = \cdot00007, \quad \text{and} \quad 2\mu = \cdot00014.$$

The difference of scale-values in x and y thus sensibly affects the fourth place of decimals. I have lumped all the four plates together (after making allowance for the rotation of the field as indicated in the paper); but each separately shows the same effect, and the close agreement testifies to the success of Mr. Jacoby's ingenious method. A little more care in the final step would have spared him the false conclusion.

Differences of scale-value of this kind are shown by the Oxford plates. In a paper on optical distortion of a different kind (*Mon. Not.* lix. p. 438) the mention of this fact was overlooked, and I am obliged to Mr. Jacoby for drawing attention to the oversight. The information omitted shall be collected and published; for the whole subject is of vital interest. Moreover, there is one point which calls for special examination. If the telescope be rotated, *as a whole*, round its optical axis, the differences of scale-value may be due to either the object-glass or the plate. It is not easy to see how tilt of the plate could give rise to such an effect; but the safest plan would be to rotate the object-glass separately. Experiments of this kind require a little arrangement, but there is no serious difficulty in carrying them out; and the results would practically settle the question. A good deal of ink has been spent in discussing whether to assume four constants or six in the equations, and perhaps we may moderate the flow of it. The obvious advantage of using six is, of course, that it affords a valuable check on the results, which is entirely lost by using four.

I have just been looking through the 'Journals of Walter White,' Assistant-Secretary of the Royal Society (Chapman & Hall, 1898). The book might have been inspired by a circumstance mentioned under date 1849, Feb. 10:—"Mr. Moseley said today that if all the *on dits* of science were set down daily, the result would be valuable some twenty years hence." But the Journal began in 1833, and though this remark may have kept it alive, it did not start it. Whether the result can be called valuable is perhaps doubtful, but it is distinctly amusing. We get glimpses of the past:—

> 1845, January 31.—Heard Faraday lecture at the Royal Institution on the Liquefaction and Solidification of Substances commonly called gaseous. Saw for the first time in my life Carbonic Acid in a solid state: the method by which he produces an inconceivable degree of cold was beautifully explained and illustrated.
>
> 1845, November 29.—It is said that the Oxford professors rejoice at his (Buckland's) promotion, saying "Thank God he is gone, if we could only get rid of Daubeny we should not have a scientific man amongst us."

An entry on 1850, May 1, gives a vivid account of Airy's discovery of the compensation of the compass of iron ships. An interesting ceremony is sketched under date 1858, Sept. 21:—

> To Grantham in charge of the Royal Society's Newton Telescope. A pleasant journey, and all new to me. The telescope was carried in the procession by some of the Grammar School boys. Lord Brougham delivered the oration to inaugurate the statue of Newton. It was a rare intellectual treat.

There is a good deal about Tennyson, and a few pages about the reception of Darwin's 'Origin of Species.' Some of the anecdotes are good, *e. g.* one of Carlyle, who "at the far end of the table discoursed so long on silence that no one else could get a chance to be heard." And there are some entries about living persons which the Editor had better have suppressed, for I fear that they may induce people like myself to read the book.

A BIOGRAPHY of a very different type, for which a second-hand bookseller asked the small sum of eighteen pence, is Miss Seward's 'Life of Erasmus Darwin' (1804). I believe that Charles Darwin showed conclusively in a later work that, as a piece of character drawing, Miss Seward's picture of his grandfather is faulty; but I am at the moment only concerned with two poetical extracts quoted in the work. The first treats of the birth of the Moon, which seems to have arrested the attention of more Darwins than one, though in the course of a century or so the family traditions have been somewhat modified:—

> When rose the continents, and sunk the main,
> And Earth's huge sphere, exploding, burst in twain,
> Gnomes, how you gaz'd, when from her wounded side
> Where now the South-sea rolls its waste of tide
> Rose, on swift wheels, the Moon's refulgent car,
> Circling the solar orb, a sister star;
> Dimpled with vales, with shining hills emboss'd
> And roll'd round Earth her airless realms of frost.

THE second extract indicates that a good deal was expected of balloons in those early days. The whole poem is called "The Botanic Garden," and this is from the second canto of the second part, which is called "Loves of the Plants": but how it weaves into the plot I have not yet had an opportunity of finding out.

> Rise, great Mongolfier! urge thy venturous flight
> High o'er the Moon's pale, ice-reflected light;
> High o'er the pearly star, whose beamy horn
> Hangs in the east, gay harbinger of morn;
> Leave the red eye of Mars on rapid wing,
> Jove's silver guards, and Saturn's dusky ring:
> Leave the fair beams, that, issuing from afar,
> Play, with new lustre, round the Georgian star;
> Shun, with strong oars, the Sun's attractive throne,
> The sparkling zodiac, and the milky zone,
> Where headlong comets, with increasing force,
> Through other systems bend their blazing course!
> For thee Cassiope her chair withdraws,
> For thee the Bear retracts his shaggy paws.
> High o'er the north thy golden orb shall roll,
> And blaze eternal round the wondering pole.
> So Argo, rising from the southern main,
> Lights with new stars the blue, etherial plain;
> With favoring beams the mariner protects,
> And the bold course, which first it steer'd, directs.

Miss Seward herself several times comments on astronomical topics. Thus she points out Cowper's (mis-?) translation of Homer:—

> As when around the clear, bright Moon, the stars
> Shine in full splendour.

Of more general interest, now that the Education Bill is in all our minds, is the story of a gentleman (the same, I believe, who wrote 'Sandford and Merton') who had peculiar views of female education:—

> His experiments had not the success he wished and expected. Her spirit could not be armed against the dread of pain and the appearance of danger. When he dropped melted sealing-wax upon her arms she did not endure it heroically, nor when he fired pistols at her petticoats, which she believed to be charged with balls, could she help starting aside, or suppress her screams.

It is pleasant to be able to add that these experiments were soon relinquished, and that the subject of them ultimately ended her days happily on the central meridian. In 1804 it is recorded that she "yet lives with the good Dr. Burney of Greenwich as his housekeeper and assistant in the cares of his academy."

At the present moment (May 23) it is not known who will win the Derby; but I see that the Derby Selling Handicap was won yesterday by RAYLEIGH, by St. Angelo—*Chemistry*; and that *Induction* was only third. Lower down the column lists are given for the Ascot Meeting, and we shall follow with interest the doings of *Chacornac*, *Refractor*, and *Rising-Glass*; but with special anxiety the successes of *Spectrum*, whose jockey is *Huggins*!

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H. P. HOLLIS, B.A., F.R.A.S.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 June 13.

Dr. J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. Since the last Meeting 68 presents have been received, and of these may be specially mentioned three volumes presented by M. Bigourdan, of the Paris Observatory, namely, his edition of Pingré's 'Annales Célestes du Dix-septième Siècle,' and two volumes of his observations of Nebulæ. From Prof. Wislicenus we have received the *Astronomischen Jahresbericht* for the year 1901, and from the Solar Physics Observatory a catalogue of 470 of the brighter stars, classified according to their chemistry. We have also received from the Dutch Eclipse Committee a preliminary report of the Dutch Expedition to Sumatra.

The President. I am sure you will pass a vote of thanks to the Donors, especially to M. Bigourdan, who, I am glad to say, is here to-night, and will, I believe, make some remarks to the Society on the subject of his observations of Nebulæ.

M. Bigourdan. J'ai l'honneur de présenter à la Société Royale Astronomique deux volumes qui renferment une partie de mes observations de nébuleuses, observations auxquelles j'ai consacré presque tout mon temps depuis plus de 18 années. Permettez-moi d'indiquer rapidement le but que j'ai poursuivi en entreprenant ce travail, les moyens qui ont été employés, et enfin son état actuel d'avancement.

Il est à peine nécessaire de rappeler quel rôle capital jouent les nébuleuses dans les théories cosmogoniques: toutes font naître le système solaire, ou même un plus ou moins grand nombre de

systèmes stellaires, d'une masse nébuleuse excessivement étendue, animée à l'origine de mouvements tourbillonnaires, et qui se condenserait graduellement sous l'influence de l'attraction mutuelle de ses particules.

Pour vérifier ces hypothèses il serait très utile de connaître les distances des nébuleuses, c'est à dire de déterminer directement leurs parallaxes; mais cela est généralement impossible, à cause de l'aspect diffus de la plupart de ces corps célestes. Et nous sommes forcés d'avoir recours à un procédé indirect, celui qui est basé sur la connaissance de leurs mouvements propres.

Parsuite il est important de mesurer les positions des nébuleuses avec toute la précision que comporte leur aspect. D'ailleurs, même un résultat négatif, même l'absence de mouvement propre, supposée constatée pour un assez grand nombre de nébuleuses, peut encore donner une idée de la distance de ces astres; en effet, la spectroscopie montre que les nébuleuses sont animés, en moyenne, de vitesses analogues à celles des étoiles. Par suite, de la fixité *apparente* des nébuleuses au fond du ciel on pourra déduire une valeur minima de leurs distances à la terre.

D'un autre côté, les mesures précises de nébuleuses étendront nos connaissances relativement au véritable mouvement du système solaire dans l'espace. Car, si le soleil n'est que l'une des innombrables étoiles qui forment la voie lactée, son déplacement séculaire (qui n'a été déduit jusqu'ici que de l'ensemble des mouvements propres des étoiles), n'est qu'un mouvement relatif dans la voie lactée; et le mouvement propre de celle-ci ne peut-être déterminé qu'à l'aide de points de repère beaucoup plus éloignés, par l'ensemble des nébuleuses.

Telles sont, du moins, les considérations qui m'ont fait entreprendre les mesures différentielles et précises de toutes les nébuleuses observables à Paris et qui sont au nombre d'environ 6500: l'équatorial employé a une ouverture de $0^{m},305$ ou environ 12 inches.

Les astronomes qui jusqu'ici s'étaient attachés à cette branche de l'astronomie n'avaient fait porter leurs observations que sur 500 à 600 nébuleuses, d'ailleurs les plus brillantes, les plus faciles à observer. Ainsi, sans compter Lord Rosse et ses aides,

Schönfeld en a mesuré........	489
Schultz „	500
d'Engelhardt „	500 environ.

Pour entreprendre la mesure d'un nombre de nébuleuses 10 à 12 fois plus grand, et comprenant les plus faibles, il parut nécessaire de changer la méthode ordinaire d'observation, qui est celle que l'on emploie généralement pour les comètes; et j'ai adopté celle qui est en usage pour les étoiles doubles, c'est à dire dans laquelle l'objet est rapporté à son étoile de comparaison par la mesure de l'angle de position et de la distance; par cette méthode j'ai pu avancer 4 à 5 fois plus rapidement que par l'ancienne, sans d'ailleurs rien sacrifier de la précision *. On peut seulement objecter que les étoiles de comparaison ne sont pas toujours déterminées; mais elles le seront prochainement et par des observations contemporaires grâce à la description photographique du ciel.

Enfin l'état d'avancement de ce travail est indiqué par le diagramme placé tous vos yeux. (A diagram was shown on the screen.)

Ces parties plus foncés correspondent à la partie déjà publiée et les deux volumes que je présente ici comprennent la partie de $14^h\ 0^m$ à $24^h\ 0^m$.

Il peut paraître singulier que la publication ait ainsi été commencée en quelque sorte par la fin ; il est nécessaire de donner deux mots d'explication à ce sujet.

Il aurait été désirable que l'ensemble de ce travail fût publié en ouvrage séparé, individuel. Mais la forme habituelle des publications de l'Observatoire de Paris ne s'y prétait guère, comme il en serait sans doute de même avec les publications actuelles de votre Observatoire de Greenwich.

D'ailleurs quand la durée prévue d'un travail est de 20 ans, et peut en atteindre 30, il serait imprudent d'attendre son entier achèvement pour le soustraire, par la publication, à toutes les chances de destruction.

Pour ces raisons, on s'est arrêté à une publication par fascicules, à peu prés un pour chaque heure d'ascension droite; et chaque fascicule, inséré dans les Observations de Paris, est publié quand les mesures correspondantes sont bien complètes : ainsi le fascicule de 14^h se trouve dans les Observations de 1899; celui de 15^h dans les Observations de 1884, etc.

On a fait de chaque fascicule un tirage à part, mais malheureusement à un petit nombre d'exemplaires, et c'est en réunissant ces tirages à part que l'on a formé quelques volumes analogues à ceux que je présente ici. La partie de 14^h à 24^h ayant été terminée la première, c'est par la fin que la publication s'est trouvée commencée.

Pour la même raison aussi, les deux volumes que je présente n'ont pas d'Introduction : celle-ci, paraîtra la dernière et contiendra toutes les données nécessaires pour reprendre complètement les diverses réductions. En attendant, une sorte d'introduction provisoire a été donné dans les Observations de Paris de 1884.

Pour donner un idée du contenu de la publication, voici la reproduction de deux pages relatives à l'heure : elles renferment 18 colonnes dans lesquelles se trouve réuni tout ce qui est relatif à chaque nébuleuse.

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* Il a même été possible d'étendre le programme primitif, et qui ne comportait qu'une seule mesure du chaque nébuleuse : le plus souvent chacun de ces astres a été mesuré deux ou même trois fois

La publication des mesures formera 5 volumes. Restera ensuite la discussion assez délicate des poids à donner aux diverses mesures d'une même nébuleuse pour conclure définitivement les différences néb. — ✱.

Enfin je me propose de former plus tard un catalogue général de nébuleuses, en donnant autant que possible leurs positions exactes, au lieu des seules positions approchées, données par les catalogues de nébuleuses publiés jusqu'ici.

Je n'ajouterai que quelques mots en présentant un autre volume, les Annales célestes du 17^e Siècle, de Pingré, l'auteur connu d'une Cométographie.

Cet ouvrage, auquel Pingré avait travaillé longtemps, dans lequel il avait réuni et souvent discuté les observations faites de 1600 à 1700, avait été perdu, quoique l'impression en eût été commencée. J'ai retrouvé successivement un exemplaire, le seul connu, de la partie imprimée ; puis le reste du manuscrit.

L'Académie des Sciences de Paris a pensé que la publication de cet ouvrage pourait encore être utile, et c'est sous ses auspices que j'en ai surveillé l'impression.

Enfin je prie la Société Royale Astronomique d'accepter un petit mémoire sur la prédiction des occultations d'étoiles par la Lune ; les tables qu'il renferme peuvent être utile également aux astronomes et aux voyageurs qui explorent des pays inconnus.

Prof. Turner. I am sure we have all listened with great interest to M. Bigourdan's remarks. Can he tell us how much of Pingré's MS. remains for publication?

M. Bigourdan. Le manuscrit de 'l'Histoire céleste du 17^e Siècle' comprend, comme Introduction des observations faites par Tycho dans sa jeunesse et qui n'ont pas été imprimées par Albertus Curtius. En outre Pingré avait relevé beaucoup d'erreurs dans la publication de ce dernier. Mais cette Introduction n'a pas été donnée dans l'ouvrage que j'ai honneur de vous présenter.

Mr. Knobel. I am sure we shall all examine with very great interest the work which M. Bigourdan has presented to us, and which is the first instalment of a great publication. M. Bigourdan has referred to the first observations of Schultz, and to the micrometer which he used. I would ask M. Bigourdan whether Schultz made any measures with a ring-micrometer.

M. Bigourdan. Non ; Schultz a employé un micromètre filaire.

Dr. A. M. W. Downing (presenting a paper on the Distribution of the Stars in the Cape Photographic Durchmusterung). I may say that though I have been ploughing a furrow it is not a lonely furrow, because after I had begun my work I came across a memoir by M. Stratonoff, of the Tashkent Observatory, on the same subject. However, M. Stratonoff had carried out his work on somewhat different lines to those on which I was working, so I thought it would be of interest to astronomers if I continued my paper. The publication of the Cape photographic

Durchmusterung was of great interest, as it enabled us for the first time to study the distribution of stars by a method free from some disabilities which were attached to the old method of eye-observing, although it had some of its own. For instance, the photographic method was far more efficient than eye-observing in registering stars in the more crowded portions of the heavens, where eye-observation was generally found to fail; but, on the other hand, the photographic magnitudes were probably different to those found by eye, so that pending the publication of the further discussions of the photographic magnitudes which was expected from the authors, I have adopted the magnitudes given in the catalogue throughout my paper. The most laborious part of the work was to count the stars of the different magnitudes as they occurred in the different zones. As there were more than 450,000 stars in the survey, the amount of labour entailed in this will be easily realized. To facilitate matters, I divided the stars into magnitude groups, taking first of all stars down to 6·2, and then going by half-magnitudes from 6·7 to 10·2, and after that I took all the stars from 10·3 together. I will write on the board some of the salient results, which give an indication of the way in which the star-density increases as the brightness of the stars diminishes. It will be remarked that the survey was supposed to be complete to magnitude 9·2 only, and that was apparently borne out in the diminution of the ratio when we proceed to a fainter magnitude than 9·2. That must not be taken as evidence of any thinning-out in the stars, but merely that the survey is incomplete beyond that limit of magnitude. An important point is that the density decreases regularly in passing from the galaxy to its pole, and there is no great jump, so that the galaxy appears to be part of the general universe of the stars. There appears to be a greater preponderance of stars in the plane of the galaxy in the photographic than there is in the visual survey, to which fact Prof. Kapteyn has called attention.

The President. I should like to ask Dr. Downing whether there are any general conclusions which the investigation has brought to light; that is to say, whether he has made anything in the way of discovery independently of those Prof. Kapteyn suggested, or of what one would have thought he could have laid hold of?

Dr. Downing. The only general conclusion to be drawn is as to the general form of the universe of stars. Also there is an evident systematic discordance between the photographic and the visual magnitudes.

Prof. Turner. Have you taken any account of this evident difference?

Dr. Downing. No, I have only taken the magnitudes as they are given in the Durchmusterung.

Mr. Hinks. There is one point I would like to ask a question about, and that is whether there is any possibility of effect coming in such as Prof. Turner found on his plates at Oxford owing to the curvature of the objective. I am interested to know whether Dr. Downing has considered this point, and whether such an effect could occur in the case of a portrait-lens, as Prof. Turner has shown it undoubtedly does in the case of the astrographic telescopes. I mean that the field of the portrait-lens is so large that one might get systematic errors depending upon the way in which the centres of the plates are distributed.

Dr. Downing. You mean from the position of the star, with reference to the centre of the plate? That has been discussed by Prof. Kapteyn in the introduction to the Catalogue.

Mr. Thackeray. It may not be within the knowledge of the Society generally that the Astronomer Royal some two years ago borrowed the original books of Groombridge's observations for the purpose of re-reducing them in order to compare them with the results of the Ten-Year Catalogue made at Greenwich in 1890, with the object of deducing proper motions, and since we have had those papers the reductions have been steadily going on. Before I say anything on that point it may, perhaps, be well to call the attention of the Society to Groombridge himself and his instrument. Groombridge was a very remarkable man. Born in the year 1745 at Goudhurst, in Kent, he was apprenticed to a draper. His employer retiring from business about the same time as his apprenticeship was finished, he for some time carried on the business in the same house, but afterwards became a West-India merchant, and in the course of years he prospered exceedingly. For some years he continued to live at Goudhurst, and, as he was always devoted to astronomy, he built for himself a small observatory. In the year 1802, when he was 47 years old, he removed to Blackheath, and there built for himself an observing-room and bought the well-known transit-circle with which his name was identified, and which Sir G. Airy described as being the finest in the world at the time of its erection and for several years afterwards. It was not, however, until the summer of 1806 that he first began his observations for his circumpolar catalogue, and he was then at the age of 51. It was at this period of life, while he punctually attended to his business in town, that during his leisure hours he made the observations on which his catalogue was founded. During the years 1806–1815 he made 25,000 observations—when he was between 51 and 60 years of age—and this, after he had made his fortune, was the laborious amusement of his well-deserved leisure. About the year 1815 he ceased to observe stars, in order to give more time to the reduction of the observations he had already made, but he observed planets up to the year 1824. In 1827 he had an attack of paralysis, from which he never completely recovered, and died in 1832, in the 78th year of his age.

Groombridge's instrument, a reversible transit-circle, 5 feet long, 3½ inches aperture, with two circles four feet in diameter, graduated by Troughton to every 5′, was mounted on two stone piers in a room close to his dining-room, so that he could easily, as he frequently did, leave his dinner, step into his observatory, make his observation, and return to his dinner. For the adjustment of his instrument Groombridge relied on observations of a mark and adjusting-screws. It has been pretty generally assumed that this mark was slightly out of the meridian, and that the catalogue-places on this account require a general correction; but the re-reduction gives no countenance to such a proposition, merely showing that the accidental error was large. The Right Ascension and North Polar Distances are both differential, and depend on the places of Newcomb's Fundamental Catalogue, the idea of the re-reduction being to place Groombridge's stars proper on the same plane of accuracy as those in the Fundamental Catalogue. It is interesting to note that 18 double transits of Polaris made by Groombridge give the place of Polaris almost identical with that adopted from Newcomb.

Mr. Dyson. I should like to add one or two remarks, as I have been associated with Mr. Thackeray in these re-reductions. Mr. Thackeray has said that there are 25,000 observations which had to be reduced, and I should like to add that in the two years since Mr. Thackeray has had this in hand all but 5000 observations have been reduced, and these he hopes to finish in a month or two. With regard to the reductions, the entries were made by Groombridge into two very neat books. We took these as being the earliest records we could get, and used Newcomb's Fundamental Catalogue as a standard of reference throughout. The second remark I would make is to prevent any misapprehension. Mr. Thackeray spoke of the large discrepancies in Groombridge's observations, but did not sufficiently convey the idea that these discrepancies were only of occasional occurrence. I think the Astronomer Royal has the intention of publishing the ledgers, as well as the revised catalogue, so that if anyone thought the position of a particular star was in error he could consult the ledgers. The paper of Mr. Thackeray's is hardly one on which much can be said, but I think it will show what an improvement has been made on Groombridge's catalogue by a re-reduction.

Dr. Rambaut. I should like to say how much Mr. Thackeray's paper has interested me, as it must have done a number of the Fellows present, I feel sure. Those of us who have studied Airy's introduction to the authorized edition of Groombridge must be able to appreciate the necessity for work of this kind. There we learn the number of difficulties occurring in the course of the reduction, how the original reductions were spun out for 15 years or thereabouts, and were entrusted to a number of computers, and how there were some difficult circumstances attending the work of one of the persons to whom it was entrusted. It is gratifying to notice that the probable error has been so far reduced, and any calculations to improve upon it are fully justified. Am I right in thinking that the places of the stars in this re-reduced catalogue will be of a wholly differential character? I was under the impression that Groombridge had observed the Sun frequently or occasionally, and I should like to know how far those observations are capable of being used for the determination of the equinox. Or are the Right Ascensions wholly dependent upon the position of the stars in Newcomb's catalogue?

Mr. Thackeray. With reference to what Dr. Rambaut has said, I may say that there are a great many observations of the Sun, and a good many observations of small planets and Jupiter and Mars, and a few observations of the Moon, but at present we have not touched them at all. We are going to tackle them separately after we have finished the catalogue. I do not think that the observations of the Sun made at any particular time by a single observer can be held to be a valuable check on the epoch correction as deduced by Prof. Newcomb from every available source, as this particular observation is so peculiarly liable to personality, which directly affects the adopted correction.

The President. I should like to say I was very interested in the account which Mr. Thackeray gave of Groombridge. I have known him by name all my life, and I also knew that his observatory was near Greenwich, but did not know exactly where. No doubt he lived near the Royal Observatory that he might get some occasional assistance.

Mr. L. N. G. Filon read a paper on the reduction of some photographs of Comet Swift (1899 *a*) taken at the Cambridge Observatory with a portrait-lens. The photographs were taken during the months of May and June, 1899, with a portrait-lens of 5 inches aperture and 30 inches focal length, which was attached to the Northumberland equatorial. The plates, however, remained unreduced for three years because no measuring-machine was at hand. One of the chief objects aimed at was to find out the magnitude of the probable error of determination of star-places from photographs taken with a portrait-lens. The six plate-constants were found by comparing the computed with the measured co-ordinates of each of the comparison-stars for a large number of exposures, and taking the mean of these it was found that the probable error of an equation of condition was $1''\cdot23$. Of this, part is due to actual errors of measurement on the plate, part to errors in the meridian places used, and part to errors in the plate-constants, which are themselves due to the first two errors mentioned.

From certain considerations it appeared that the probable error of a measure in x was certainly less than $0''\cdot5$, and that the probable error of a computed x co-ordinate was $1''\cdot47$, which includes the errors arising from unknown proper motion, the errors of observation, and errors due to having cut off the last decimal place in the Declinations and Right Ascensions.

It was found that the probable error of a deduced star-place, as found from the measures and plate-constants, can be expressed by a formula,

$$p_x^2 + \frac{1}{n}\left(1 + \frac{2k_1^2}{k^2}\right)\left(p_x^2 + p^2\right),$$

where k and k_1 are the mean square radii of the distributions of comparison-stars and of stars whose places are required respectively.

This formula shows the great advantage of increasing the number n of the comparison-stars, and also of taking the comparison-stars well outside the stars whose position is required, so as to make k'/k small.

If $k'=k$, then in the present case $p'=1''{\cdot}07$. If $k'=\frac{1}{2}k$, which is to be recommended, $p'=0''{\cdot}83$.

We may therefore assert that portrait-lens photographs should give star-places accurate to about $0''{\cdot}8$ of arc.

The comet-places are liable to far more serious errors. The object was very faint and diffuse, and the probable error of an actual measure was enormously greater than for the stars.

The probable error of the comet-places obtained by considering the want of proportionality of the differences between the R.A. and Decl. for the various exposures on one plate to the times elapsed, amounts to about $4''$ or $5''$.

Prof. Turner. I am glad to find another worker in the field, because there is plenty of work to be done, and the more help we have the better.

Dr. Rambaut. I should like to ask with what degree of accuracy Mr. Filon was able to determine the inclination of the plates to the plane of the object-glass.

Mr. Filon. The effect of tilt of the plate depends merely upon the angular extent of the field, and not upon the actual focal length. The fact that the star-images appeared in equally good focus all round the plate made it unlikely in the present case that there could have been any tilt of great amount.

Mr. Hinks. I should like to say how pleased I am to see this piece of work presented to the Society, because I have had the pleasure of seeing it going on at Cambridge at intervals for the last two or three years, and it has seemed to me to be a very useful piece of work. The point that strikes me as valuable is that a photograph with a portrait-lens is, on occasion, worth measuring, and measuring well. One might be tempted to suppose that a small-scale photograph is not worth much trouble in measurement; but Mr. Filon has shown that if you have a portrait-lens plate which is for any reason of special interest, and no larger scale photographs are available, it is worthy of the best measuring-work you can do on it.

Mr. Hinks then read a second paper, on an experimental reduction of photographs of Eros for the determination of the Solar Parallax: combination of results from Mount Hamilton, Minneapolis, and Cambridge. In the first paper, presented to the Society last November, fifty exposures made at Cambridge were reduced by the diurnal method. This required an empirical correction to the ephemeris, which was not satisfactory. It is better to combine observations made simultaneously at stations far apart, by which method the errors of the ephemeris are practically eliminated. The Directors of the Lick and Minneapolis Observatories very kindly sent measures of some of their photographs; those from Lick would be of particular interest because they were made with the instrument which has won new fame as the Crossley Reflector, but which was famous in England twenty years ago as Dr. Common's 3-foot.

The results seemed to show that we were going to get a very good value of the solar parallax out of these Eros observations. At present there is a large discrepancy between the values; on the one hand, about 8″·76 is required by planetary theory and by recent determinations of the aberration constant, and the value 8″·80 may be deduced from all the direct observational methods. The really important point seems to be, whether the photographic observations of Eros are going to reduce the discrepancy. The author proposes, therefore, to beg the Directors of observatories with photographs of Eros taken from 1900 Nov. 7 to Nov. 15, to measure these plates and allow him to combine the results into one discussion. This should give a value of the solar parallax with a probable error as small as that of Gill's value from heliometer observations; and by concentrating attention on this small block of the work we shall be able, in a short time, to know how the photographic result is coming out, and how we shall face the task of reducing the whole mass of observations.

Prof. Turner. I should like to express my strong approval and admiration of the plan proposed by Mr. Hinks of taking a limited section of the large mass of material which has been lying by with not much done with it up to now, and seeing what can be got out of it. I hope Mr. Hinks will have his limiting dates advertised as much as possible, because if he can get all the observations between those dates thoroughly threshed out we shall know much better how to put a plan for reducing the whole of the material into execution.

The President. There is a paper on the list to-night by *Mr. H. C. Plummer*, on the principle of the Arithmetic Mean, which I have had much pleasure in reading; but, as its title shows, it is an arithmetical paper, and it would not be possible or practicable to give a succinct account to anyone who has not thought much about the subject before hand.

Mr. Dyson. Of the other papers there are three on the observations of Nova Persei—one from the Radcliffe Observatory, one by

Mr. Backhouse, and one by *Mr. Stanley Williams.* There is also a short note from the Perth Observatory on observations of the Total Eclipse of the Moon, 1902 April 22, and another by *Mr. Gledhill* on observations of Jupiter made at Mr. Crossley's observatory, Bermerside, Halifax, in 1901; and there is also a short paper by *Mr. C. S. Howe* on a dark reticle.

The following papers were announced and partly read:—

J. Gledhill. "Observations of Jupiter made at Mr. Crossley's Observatory, Bermerside, Halifax, in 1901."

A. Stanley Williams. "Further Observations of the New Star in Perseus."

C. S. Howe. "A Dark Reticle."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Mars, 1902–03."

A. M. W. Downing. "On the Distribution of the Stars in the Cape Photographic Durchmusterung."

L. N. G. Filon. "Reductions of Photographs of Swift's Comet (*a* 1899), taken at Cambridge University with a Portrait-lens."

Perth Observatory, Western Australia. "Observations of the Total Eclipse of the Moon," 1902 April 22."

H. C. Plummer. "On the Principle of the Arithmetic Mean."

A. R. Hinks. "Experimental Reduction of Photographs of Eros for the Determination of Solar Parallax. Second Paper: Combination of Results from Mount Hamilton, Minneapolis, and Cambridge."

Radcliffe Observatory, Oxford. "Further Observations of the New Star in Perseus." (Communicated by the Radcliffe Observer.)

The following gentleman was elected a Fellow of the Society:—

H. B. Adames, 5 West Street, Chichester, Sussex.

The following Candidate was proposed for election as a Fellow of the Society:—

G. McKenzie Knight, 27 King Henry's Road, West Hampstead, N.W. (proposed by W. E. Besley).

THE BRITISH ASTRONOMICAL ASSOCIATION.

President G. M. Seabroke in the Chair.

Secretaries: A. C. D. Crommelin and
J. G. Petrie.

The seventh Ordinary Meeting of the twelfth Session of the British Astronomical Association was held at Sion College, Victoria Embankment, on Wednesday, May 28th.

Among the presents announced was a gift from the Rev. E. Ledger of 125 negatives to the lantern-slide department.

Capt. Molesworth read a paper on the Annular Solar Eclipse of November 11th last, as seen at Trincomali, Ceylon. The eclipse party was composed of officers stationed at Trincomali and from H.M.S. '*Highflyer*,' the flagship, Capt. Molesworth being the only one of the party with any previous experience. The instruments used were a 12½ Calver equatorial (clock-driven), carrying on the same mounting a Marion portrait-lens of 6 inches aperture and about 31 inches focus, a small equatorial, on which was mounted a Cooke lens with Dallmeyer telephoto attachment. Owing partly to the cloudy and hazy weather, and to mishaps with the improvised apparatus, the observations were, generally speaking, a failure. Three slides were afterwards thrown upon the screen, one of the annular phase from a negative taken with the 12½-inch, and two others showing the eclipse party at work.

Mr. Maunder felt sure they must all sympathize with Capt. Molesworth in the difficulties which he had experienced in observing this eclipse last November. At the very best eclipse work was extremely trying even under the most favourable circumstances. Perhaps the next best thing to very good conditions was when the conditions were extremely bad and nothing could be seen, for then the observer's mind was set at rest. He, the speaker, had been on five eclipse expeditions, and he thought the feeling experienced on each occasion was one of wonder that anybody who could stop quietly at home should, of his own free will, go through the worry which eclipse work involved.

Mr. Crommelin thought that the fact that Capt. Molesworth had obtained traces of the corona on this occasion when the annulus was as wide as they could have it, for it was practically a record for width of annulus, made it quite hopeful than in some annular eclipses, when the annulus was narrow, a good deal might be done on the corona, particularly in the eclipse of 1912, because, although there would be two or three seconds of totality in Portugal, it might be described as an annular one.

The President thought Capt. Molesworth hardly did himself justice when he described his paper as a record of failures. From failures a good deal had been learned for guidance in future work.

Mr. G. T. Chambers asked whether any member of the Association had noticed anything abnormal last week in the appearance of Moon. He and some members of his family had noticed a peculiar ruddy tint when it was well above the horizon, and the question occurred to him, "Could this have been in anyway due to the earthquakes in the West Indies?" Many present would, no doubt remember the remarkable sunsets of 1883, which were associated, according to a well accepted belief, with the eruptions of Krakatoa.

Mr. Goodacre said he had not observed the Moon telescopically during the last apparition, but he had observed it visually, and there certainly was an amount of ruddiness about it such as Mr. Chambers mentioned; at the same time it did not occur to him to connect this at all with the volcanic eruptions in Martinique or St. Vincent. He rather put it down to the great southern declination which the Moon had about this time of the year, and the obscuration by London smoke in which it was immersed by reason of its low altitude.

Mr. Crommelin had noted the Moon on that day week and had been struck by a distinctly greenish tinge. The question as to whether it had anything to do with the recent eruptions had occurred to him, but it seemed to him that it was a little too soon to expect the dust by that time. In the case of Krakatoa, the abnormal sunsets began about two months after the eruption. As regarded these eruptions there was one point that might be of astronomical interest, and that was that the cataclysm that overwhelmed the town of St. Pierre happened exactly at New Moon. Moreover the Moon was in perigee at the time and an unusually near perigee. That might have been the reason for the cataclysm coming at that moment; they might well imagine that the tidal action of the Moon did not affect the ocean only, but also the liquid matter of the interior of the Earth; and if so, that the tidal action would be at a maximum at the time—more especially as the Sun and Moon were roughly in the same North Declination as the parallel of Martinique, so that they would pass practically overhead, and on all these accounts the tidal action would be at a maximum at the time at Martinique.

Mr. Lynn asked whether the Moon was in perigee at the time of the Krakatoa eruption.

Mr. Crommelin replied that he had not looked up the point.

Mr. Saunder said he had not been fortunate enough to observe the Moon during the past week owing to persistent clouds; but his recollection of the appearance of the Moon after the Krakatoa eruption was that it was distinctly blue or green for a good many nights. He did not remember any ruddy appearance then.

The President. The sunset was red.

Mr. Saunder. Yes, but the Moon was blue or green.

Mr. Newbegin remarked that Mr. Chambers had raised an important question, and members should all be on the watch for any effects that might be visible, either in the Moon or in the sunsets.

Mr. London said that living about eighty miles from London he had observed for several days that both the sunsets and sunrises were accompanied by a peculiar golden haze.

Mr. W. T. Lynn read a paper on "The Causes of Variability in the Stars." The writer said that probably the balance of opinion was that variability was due to some physical change in the stars themselves, and that its analogies were to be found in the eleven-year period of spots, now recognized though not accounted for, on our Sun. If so, however, it must be on a much larger scale in nearly all the variable stars.

Mr. Maunder remarked that the long-period variables were exceedingly difficult to explain. In the first place, there was a large number whose period of variability was very nearly that of a terrestrial year. A period of that length seemed scarcely likely to be one of simple rotation, nor did it seem likely to be analogous to the eleven-year period of sun-spots. Then they had to bear in mind that the amount of variation was in some cases a very large one. For instance, in *o* Ceti they had an extreme variation of brightness. Sometimes the star was a thousand times as bright as it was at other times. Then, again, in not a few of the long-period variables it was found that at maximum they showed the hydrogen lines bright. He did not think that as yet they had hit upon a satisfactory solution of the difficulty. He did not see how Mr. Lynn's suggestion would account for the case of *o* Ceti.

Mr. Lynn. Surely *o* Ceti is an exceptional case?

Mr. Maunder. True, but there are some cases where there is an even greater range than *o* Ceti.

The President was afraid he was very ignorant of variable stars, but he believed there were four classes. Firstly, the Algol variables were pretty well accounted for by the eclipse theory; but as to clases 2 and 3, regular variables of considerable variability and those of slight variability, perhaps Mr. Lynn's theory might account for the slight variation of class 3, such as a few tenths of a magnitude or something of that order, but there appeared at present to be no satisfactory theory to account for the *o* Ceti type of variables.

Mr. Walter F. Gale (Newcastle, N.S.W.) contributed some notes regarding the planet Jupiter. He said that taking the 1901 opposition of the planet throughout, the most remarkable features appeared to have been the fading of the N. equatorial belt and the increase of density and sharpness of outline of the N. temperate belt. The N. tropical zone had also exhibited a marked increase of brightness. The N. temperate belt was usually seen distinctly double.

Mr. Crommelin read a paper by *Major L. A. Eddie* (written from Naaurpoort, Cape Colony, April 23, 1902) on "Varying Colours during the Progress of a Lunar Eclipse." He described the gradual changes in colour and tint in the eclipse of April 22 last, and said that one rather remarkable feature was that, whereas from an early stage of the eclipse the shaded portions reflected a coppery colour which gradually deepened to a ruddy brown, and finally to a dull red, yet, shortly after totality had ceased and the illuminated cresent had waxed to the size of a three days' old Moon, the whole of the remainder of the lunar disc became invisible as if totally obliterated from the heavens.

Capt. Molesworth said he also observed the eclipse from the Indian Ocean, and he could corroborate what Major Eddie said as to the darkness of the eclipsed portion of the Moon during the partial phase.

Mr. Goodacre said that observers in this country agreed pretty much that during the latter part of the eclipse the Moon became entirely invisible so far as regarded the obscured portion.

Mr. Petrie read a paper by *Mr. G. J. Burns, B.Sc.*, on the "Origin of Lunar Formations," which suggested that it might be possible to explain the origin of these formations by assuming that at one time there was a hot viscous nucleus covered by a cool solid crust; then the lighter portions of the viscous matter gradually made their way towards the surface till they came to the underside of the crust; that crust in some places yielded to the pressure underneath without rupturing, so that a mountain resulted; while in other places the crust ruptured and craters were formed. Mr. Burns described some experiments he had made with molten pitch, and a light fluid, such as alcohol, benzine, or ammonia, bearing out his theory.

Mr. Goodacre thought that, bearing in mind the small amount of gravity on the surface of the Moon as compared with that on the Earth—about one sixth—and also the further fact that the material of which the Moon is composed is in the aggregate much lighter than that of the Earth, they had sufficiently safe ground to go upon in assuming that the present appearance of the lunar surface was simply the result of volcanic energy.

Some lantern-slides of private observatories were then thrown on the screen and explained.

The President announced that, owing to the Coronation, the next Meeting of the Association had been postponed to Wednesday, July the 9th.

ROYAL METEOROLOGICAL SOCIETY.

THE second afternoon Meeting of the present Session was held on Wednesday the 18th instant, at the Society's rooms, 70 Victoria Street, Westminster, *Mr. R. Inwards*, Vice-President, in the Chair.

Mr. F. C. Bayard read a paper on "English Climatology, 1891–1900," which is a discussion of the climatological data printed in the Meteorological Record. In 1874 the Royal Meteorological Society commenced the organization of a series of stations at which the observations are made twice a day on a uniform plan, so that the results may be strictly comparable with each other. In addition to these, the Society in 1880 organized another class of stations, termed Climatological, at which the observations are made once a day, viz. at 9 A.M. Mr. Bayard on a former occasion worked up the results from these climatological stations for the 10 years 1881–1890, and in the present paper he gives the averages from 69 stations for the 10 years 1891–1900. The elements dealt with are temperature, relative humidity, amount of cloud, rainfall, and rainy days, and the results are a valuable contribution to the climatology of the British Isles.

A paper by *Mr. W. L. Dallas*, on "Earth Temperature Observations recorded in Upper India," was also read, in which the author discussed the observations made on the temperature of the soil at three stations, viz.:—Lahore, the capital of the Punjab; Dehra Dun, in the north-west of the North-Western Provinces; and Jaipur, the capital of the native State of that name. The observations, which were made at depths varying from 4 inches to 45½ feet below the surface, extended from 1884 to 1899.

Messier's Nebulæ.

IN the year 1771, Messier, the famous discoverer of comets, published a catalogue of 68 nebulæ in the Memoirs of the French Academy of Science. In the years 1781–82 this list was republished in the *Connaissance des Temps* and 35 other nebulæ were added, thus bringing the total up to 103 nebulæ. Most of these nebulæ have been carefully reobserved since Messier's time, and some of them have proved to be most wonderful and interesting objects. As these observations and descriptions are scattered through various volumes, some of them almost inaccessible to the general reader, I have compiled the following list with short descriptions of the appearance and character of each nebula. The positions are given for 1900·0. There are a few of Messier's nebulæ of which I have been unable to find any account. These are Nos. 40, 45, 48, and 102.

Messier 1. R.A. $5^h\ 28^m \cdot 5$, N. 21° 57′.—The famous "crab nebula" in Taurus. It lies about 1 degree north preceding the star ζ Tauri. It was first seen by Bevis in 1731. It was again seen by Messier in 1758 while observing the comet of that year, and its rediscovery induced him to form his catalogue of nebulæ, to help observers in distinguishing these objects from comets. Sir John Herschel thought that it was a cluster of stars at a distance "of about the 980th order," that is 980 times the distance of Capella or Vega. Lord Rosse's telescope is said to have resolved it into stars, but photographs taken by Dr. Roberts in 1892 and 1896 show a very nebulous appearance, and with but little of the "crab-like" form depicted in Lord Rosse's drawing.

M. 2. R.A. $21^h\ 28^m.3$, S. 1° 15′.—In Aquarius, about 5° north of the star β Aquarii. It is a globular cluster of 5′ or 6′ in diameter. Sir John Herschel compared it to a heap of white sand; and Admiral Smyth says: "This magnificent ball of stars condenses to the centre and presents so fine a spherical form that imagination cannot but picture the inconceivable brilliance of their visible heavens, to its animated myriads." But that each of these points should have planets revolving round them seems very doubtful. Sir William Herschel with his 40-feet telescope could see the individual stars even in the centre of the cluster. A photograph by Dr. Roberts, taken in 1891, shows the centre of the cluster involved in dense nebulosity, and he thinks that it was probably evolved from a spiral nebula. The stars composing the cluster are very faint, probably not above the 15th magnitude. Seen as a star it was measured at Harvard Observatory as 7·69 magnitude. Assuming these magnitudes as correct, I find that the cluster contains about 800 stars.

M. 3. $13^h\ 37^m.6$, N. 28° 53′.—A fine globular cluster in Canes Venatici, about 12° north preceding Arcturus. Messier, who discovered it in 1764, described it as "a nebula without a star, brilliant and round." Sir William Herschel, with his 20-feet reflector, found it "a beautiful cluster of stars 5′ or 6′ in diameter." Sir John Herschel describes it as very large and bright, and the stars about 11th to 15th magnitude. Smyth called it "a noble object," and thought it contained "not less than 1000 small stars." Lord Rosse saw it as a cluster blazing in the centre with rays of stars running out from it on all sides. A photograph by Dr. Roberts taken in May 1891 "confirms the general descriptions" of previous observers. No less than 132 variable stars have been detected among the outliers of this cluster!

M. 4. $16^h\ 17^m.5$, S. 26° 17′.—A large but rather faint nebula. It lies about 1½° preceding the bright star Antares. It was discovered by Messier in 1764, and in 1783 Sir William Herschel found it resolvable into stars. Admiral Smyth describes it as "a compressed mass of small stars . . . with outliers and a few small stellar companions in the field."

M. 5. $15^h\ 13^m.5$, N. 2° 27′.—A globular cluster of stars of 11th to 15th magnitudes, lying closely north preceding the 5th magnitude star 5 Serpentis. Messier was "certain that it contained no star"! Sir William Herschel resolved it and estimated the number at 200, but it probably contains many more. Smyth describes it as "a noble mass, refreshing to the senses after sweeping for faint objects." A photograph taken by Dr. Roberts in 1892 shows the stars to 15th magnitude, "with dense nebulosity about the centre." No less than 85 variable stars have been detected among the outliers of this cluster.

M. 6. $17^h\ 33^m$, S. 32° 10′.—In Scorpio. Sir John Herschel describes it as "a fine, large, discrete cluster of stars, 10, 11; one star is 7 m.; one is 7·8. Fills field." From the position given by Sir John Herschel for the nebula Dunlop 612, it should be in the same low power field with M. 6, but observing in the Punjab I could only see *one* cluster near the place, with stars in zigzag lines, which agrees with Sir John Herschel's description of Dunlop 612.

M. 7. $17^h\ 44^m$, S. 30° 39'.—Sir John Herschel describes it as "A highly condensed nebulous mass, 3' diam., or an irregular round nebula; pretty much brighter in the middle; resolvable."

M. 8. $17^h\ 57^m{\cdot}6$, S. 24° 22'.—A very fine object in the Milky Way in Sagittarius. Visible to the naked eye. A beautiful drawing of it showing nebulous streaks and loops is given by Sir John Herschel in his *Cape Observations*, and he calls it "a superb nebula." There are many stars in the field. The present writer found it plainly visible to the naked eye in the Punjab sky, and a glorious object even with a 3-inch refractor. It has been photographed by Dr. Roberts*. Secchi found the spectrum that of a gaseous nebula.

M. 9. $17^h\ 13^m{\cdot}3$, S. 18° 25'.—Between η and 58 Ophiuchi; nearer to η. Messier discovered it in 1764, and described it as a nebula "unaccompanied by any star"; but it was resolved into stars by Sir W. Herschel in 1784 with his 20-feet reflector. Sir John Herschel at the Cape found it about 4' in diameter, and "resolved into stars of the 14th magnitude." Smyth says: "This fine object is composed of a myriad of minute stars, clustering into a blaze in the centre, and wonderfully aggregated, with numerous outliers seen by glimpses."

M. 10. $16^h\ 51^m{\cdot}9$, S. 3° 57'.—About 10° east of the stars δ and ϵ Ophiuchi. Sir John Herschel describes it as a "fine large cluster . . . diameter 5', with stragglers, several of which are of larger size to about 12'; all resolved into stars 11–15 magnitude; very compressed." A photograph by Dr. Roberts in June 1891 shows the cluster "nearly free from nebulosity."

M. 11. $18^h\ 45^m{\cdot}7$, S. 6° 23'.—In the so-called "Shield of Sobieski," which forms the southern portion of Aquila. Discovered by Kirch in 1681. Sir William Herschel saw stars of about 11th magnitude, divided into 5 or 6 groups, and he found it just visible to the naked eye. Sir John Herschel calls it round and rich, with stars 9–11th magnitude. Smyth compared it to "a flight of wild ducks," and says it "is a gathering of minute stars with a prominent 8th magnitude star in the middle and two following." A photograph by Dr. Roberts in 1891 agrees well with the descriptions, and shows that the cluster is free from nebulosity.

M. 12. $16^h\ 42^m{\cdot}0$, S. 1° 48.—Between δ and 41 Ophiuchi. It was discovered by Messier in 1764 and described by him as "a round nebula, unaccompanied by any star." Sir J. Herschel found it resolvable into stars of 10th magnitude. Smyth calls it

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* See *Knowledge*, June 1900.

"a fine rich globular cluster with a cortège of bright stars and many minute straggling outliers." A photograph taken by Dr. Roberts in June 1892 shows the stars visible to the centre with "a trace of nebulosity."

M. 13. $16^{\mathrm{h}}\ 38^{\mathrm{m}}{\cdot}1$, N. 36° 37′.—This is the famous globular cluster in Hercules, between η and ζ, and considered to be one of the finest of its class. It was discovered by Halley in 1714. Messier, who observed it with a 4-foot Newtonian reflector and power of 60, was "assured that it contains no star"! Sir William Herschel described it as "a most beautiful cluster of stars, exceedingly compressed in the middle and very rich." He estimated the number of stars at 14,000. Sir J. Herschel saw thousands of stars. Smyth called it "a superb object," and Dr. Nichol remarked that "Perhaps no one ever saw it for the first time through a telescope without uttering a shout of wonder." Lord Rosse found 3 dark streaks or lanes, which were also seen by Buffham. Photographs taken by Dr. Roberts in 1887 and 1895 show the dark lanes seen by Lord Rosse, and also some nebulosity. Dr. Roberts thinks that it was probably evolved from a spiral nebula. From a photograph taken in America by Mr. H. K. Palmer with an exposure of two hours he finds the number of stars in this cluster to be 5482, of which 1016 are "bright" and 4466 "faint." The dark lanes are also clearly visible in Palmer's photograph.

M. 14. $17^{\mathrm{h}}\ 32^{\mathrm{m}}{\cdot}3$, S. 3° 11′.—In Ophiuchus, between τ and 41. Sir John Herschel found it to be "a most beautiful and delicate globular cluster; not very bright, but of the finest star-dust; all well resolved.... excessively rich. All the stars equal 15 or 16 m." Lord Rosse found the stars very close and very small. A photograph taken by Dr. Roberts in August 1897 shows "curves and lines of faint stars radiating in all directions outwards from the dense cluster ... as well as vacancies, within the nebulous centre."

M. 15. $21^{\mathrm{h}}\ 25^{\mathrm{m}}{\cdot}1$, N. 11° 43′.—A globular cluster in Pegasus in a comparatively vacant space about 4° north following δ Equuleii. It was discovered in 1745 by Miraldi, who thought it contained "many stars." Sir John Herschel estimated them about 15th magnitude. Lord Rosse described it as a globular cluster with bright and faint stars. A photograph taken in Nov. 1890 by Dr. Roberts "confirms the general descriptions," and shows the stars arranged in "curves, lines, and patterns," and the centre nebulous.

M. 16. $18^{\mathrm{h}}\ 13^{\mathrm{m}}{\cdot}2$, S. 13° 49′.—South of "Sobieski's Shield." Sir John Herschel described it as a cluster containing at least 100 stars, and Smyth calls it a "A scattered but fine large cluster." A photograph taken by Dr. Roberts in August 1897 shows that it is really "a large bright *nebula* with a cluster apparently involved in it."

M. 17. $18^{\mathrm{h}}\ 15^{\mathrm{m}}{\cdot}0$, S. 16° 14′.—This is the so-called "horse-shoe" or "omega" nebula. It lies about 2½° south of M. 16. It was described by Sir John Herschel as a magnificent object, bright and very large. It has also been described and drawn by Lord Rosse, Lassell, Holden, and Trouvelot. Smyth gives a drawing of it which very much resembles a horse-shoe, but this form is nearly lost in a photograph taken by Dr. Roberts in August 1893. Sir William Huggins finds the spectrum gaseous.

M. 18. $18^h\ 14^m{\cdot}1$, S. 17° 10′.—About one degree south of the preceding, M. 17. Webb says "glorious field in a very rich vicinity; south lies a region of surpassing splendour."

M. 19. $16^h\ 56^m{\cdot}4$, S. 26° 7′.—In Ophiuchus, between 39 Ophiuchi and Antares, nearer the former. Sir W. Herschel resolved it into stars. Sir John Herschel describes it as "superb. A globular cluster; very bright; round; diam. 10′; resolved into stars 14, 15, 16 magnitude." Smyth called it "A fine insulated globular cluster of small and very compressed stars," and he says it "is near the large opening or hole, about 4° broad, in the Scorpion's body, which Sir William Herschel found almost destitute of stars."

M. 20. $17^h\ 56^m{\cdot}3$, S. 23° 2′.—The well-known "Trifid nebula," which lies closely north of the star 4 Sagittarii. A very curious object with three dark lanes radiating from the centre. It has been well drawn by Sir John Herschel and Trouvelot, and it was photographed at the Lick Observatory with the 3-feet Crossley reflector*. Although it has a very nebulous appearance, the spectrum seems to be *not* gaseous.

M. 21. $17^h\ 58^m{\cdot}7$, S. 22° 30′.—Between μ and 4 Sagittarii and a little north following M. 20. Smyth describes it as "A coarse cluster of telescopic stars in a rich gathering galaxy region."

M. 22. $18^h\ 30^m{\cdot}3$, S. 23° 59′.—About midway between μ and σ Sagittarii. It seems to have been seen by Hevelius before 1665, and it was drawn by Le Gentil in 1747. Messier, who observed it in 1764, thought that it contained no stars! Sir John Herschel describes it as "a globular cluster, very bright; very large; very much compressed, 7′ diam. The stars are of two magnitudes, viz. 15–16 magnitude and 12 m.; and, what is very remarkable, the largest of these latter are visibly reddish." And in another observation he says: "consists of stars of two sizes, 11 and 15, with none intermediate, as if it consisted of 2 layers, or one shell over another. A noble object." Webb found it "very interesting from visibility of components 10 and 11 m.†, which makes it a valuable object for common telescopes and a clue to the structure of many more distant or difficult nebulæ." Observing with a 3-inch refractor in the Punjab, the present writer found the larger stars well seen. The greater portion of the cluster is, however, nebulous with a telescope of this size.

M. 23. $17^h\ 51^m{\cdot}0$, S. 19° 0′.—Smyth describes it as "A loose cluster .... an elegant sprinkling of telescopic stars over the whole field, under a moderate magnifying-power." Webb says "a good low-power field."

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* Astronomical Society of the Pacific, June 1, 1900.

† Probably Sir John Herschel's *brighter* components.

M. 24. 18$^{\text{h}}$ 12$^{\text{m}}$·6, S. 18° 27′.—About 2° north of μ Sagittarii. Sir John Herschel describes it as pretty large and very rich, with stars of 11–20 magnitude. Lord Rosse saw some unresolved nebulous light in it, but a photograph by Dr. Roberts, taken in Feb. 1894, shows the stars free from nebulosity. On this photograph the streams of stars surrounding the centre seem to be arranged in spirals and strongly suggest that the cluster has been evolved from a spiral nebula. J. E. Gore.

[To be continued.]

Some interesting Double Stars.

Among the Struve double stars there are some which, at the present time, require measuring by reason of having arrived at a critical position where it is possible to discriminate perhaps between rectilinear motion and orbital motion, or between an orbit of long or comparatively short period. Most of these pairs are within reach of instruments of moderate size, and would well repay any attention bestowed upon them, thus :—

Σ 1785. R.A. 13$^{\text{h}}$ 44$^{\text{m}}$ 32$^{\text{s}}$, N.P.D. 62° 31′, Mags. 7·2 and 7·5.

The measures of this pair date from those made by South in 1823, and a complete list down to 1892 will be found in the *M. N.* 1892 Dec., where Burnham gives a diagram. In the *M. N.* 1893 March, Mr. Gore gives an orbit with a period of 125·5 years, which apparently induced Burnham to re-examine the measures, and the result will be found in *Astronomy and Astrophysics*, 1893 May, where he shows that it is impossible to determine an orbit, and that the utmost we can say is that the period lies somewhere between 120 and 300 years. In the same year my own note is to the effect that "a critical portion of the curve was being passed over, and in 1898 we might be able to get some more definite information." The measures are roughly :—

1830	165°	3″·5	1870	199°	2″·6
40	172	3 ·5	80	215	2 ·1
50	178	3 ·3	90	241	1 ·6
60	188	3 ·0	1900	276	1 ·3

from which it is seen that the prophecy has not turned out well, and we can now only say that the ellipse may be one of 130 years or of 300 years. The distance does not decrease so rapidly as Gore expected, for according to his orbit it would not exceed 1″·1.

This, then, is essentially a pair to be watched. There is no question as to its binary character, for Stumpe gives a Proper Motion of 0″·481 in direction 269°·95.

Σ 1757. R.A. $12^h\ 29^m\ 11^s$, N.P.D. 89° 48′, Mags. 7·8 and 8·9.

This pair is not so interesting perhaps as the last, but it has some peculiarities which make it worth following. In 1887 Gore gave an orbit of 277 years' period, which was properly taken exception to by Burnham, seeing that the whole change in position-angle amounted only to 60°. Indeed the motion from 1830 to 1890 is very regular, viz. 0″·035 along a straight line in direction 115°, which continued up to the present time gives for the pair a position-angle of 79° and distance 2″·8, which is, curiously enough, that derived from Gore's elements. I can find only a few recent measures, but these are consistent in their opposition to the distance of 2″·8. They appear to show that in 1893 the pair reached a maximum separation of 2″·6, and that there has since been a steady fall to 2″·2, while the angle agrees with the deduced angle of 79°. Hence careful measures of distance are of great importance, and can be made with small instrumental means.

Σ 1863. R.A. $14^h\ 34^m\ 41^s$, N.P.D. 38° 0′, Mags. 7·1 and 7·4.

This is a pair of a different character, and by reason of its distance 0″·7 more difficult to observe. Practically since 1828 the angle has changed from 111° to 87° and the distance from 0″·6 to 0″·7. When the observations are grouped and plotted down for every five years, the measures, so far as direction is concerned, seem to lie along a straight line, but the motion along this line is not regular, being as follows:—

0″·0060 from 1828 to 1835	0″·0050 from 1828 to 1860
0 ·0060 „ „ 1840	0 ·0038 „ „ 1880
0 ·0050 „ „ 1850	0 ·0032 „ „ 1900

Indicating that the pair form a binary system, and that the maximum elongation has been reached. This star is Piazzi XIV. 148.

Σ 1837. R.A. $14^h\ 19^m\ 10^s$, N.P.D. 101° 13′, Mags. 7·1 and 8·7.

This is a pair very similar in character to Σ 1863, the crux being in the distances. Plotting the observations and drawing a smooth curve through them, we get:—

1830	326°	1″·40	1870	311°	1″.33
40	322	1 ·48	80	308	1 ·30
50	318	1 ·50	90	306	1 ·20
60	314	1 ·40	1900	301	1 ·18

There does appear to be a real elongation about 1850, and measures of distance now are very important. The pair have a Common Proper Motion of −0″·043 in R.A. and +0″·024 in N.P.D. The magnitudes should also be noted. T. L.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

John Goodricke.

GENTLEMEN,—

When a career is cut off so early as that of John Goodricke, it is impossible to avoid feeling how much more would probably have been done had that life been prolonged. It is said that when Newton heard of the death of Roger Cotes (the first to hold the Plumian professorship at Cambridge), he exclaimed, "Had he lived longer, we should have known something!" Cotes was in his thirty-fourth year; but he of whom I speak had not completed his twenty-second on the day of his death, the 20th of April, 1786. His name will ever live in astronomy from his great discoveries of the periodicity of the variability of Algol (the true cause of which he suggested) and of the variability of δ Cephei and β Lyræ. The records of these in the *Philosophical Transactions* are too well known to need further reference to them here. Some time ago I pointed out in the *Observatory* (vol. iv. p. 181) that Goodricke had the priority over Palitzsch in his observations of Algol. My purpose now is to give some little account of Goodricke's family and antecedents, derived, of course, from the 'History of the Goodricke Family,' which was edited by Mr. C. A. Goodricke in 1885.

Henry Goodricke, who was the ancestor of the Yorkshire branch of the family, was the brother of Thomas, Bishop of Ely and Lord Chancellor of England in the reign of Edward VI. In 1542 he purchased the manor of Ribston (a place so universally known in connection with the celebrated variety of pippins), in Yorkshire, and afterwards other places in the same county, besides possessing landed property in the isle of Ely, and in Essex and London. His son and grandson, both named Richard, held at different times the office of High Sheriff of Yorkshire. Henry, eldest son of the latter, was knighted by James I. during his progress from Scotland to London in 1603. He had nine sons; the seventh was named John, succeeded to his father's estates in 1641, and was the same year created a baronet by Charles I. In the Civil Wars he took up arms on the side of the King, was taken prisoner by the Parliamentary forces and kept in the Tower till 1646, when he was released and the sequestration of his estates taken off. His only son, Sir Henry Goodricke, was the second baronet (for some time Ambassador at the Court of Spain, from which country he had to escape on account of some political troubles), was Sir John's only son by his first wife; as he left no posterity the third baronet was another Sir John, the only son of the former by his second wife, who succeeded his half-brother in 1705, but died in the same year. His eldest son, Sir Henry Goodricke, was the fourth baronet; he died in 1738, and was succeeded by his eldest son, Sir John Goodricke, born at Ribston in 1708, and grandfather of the astronomer. Sir John had only one son, Henry, who married, in 1761, a Dutch lady, Levina Benjamina Sessler, of Namur, with whom he resided several years at Groningen *, where most of his children were born, the eldest of these being John, of astronomical fame, on Sept. 17th, 1764, When Henry Goodricke returned to England he settled at York, where his son John developed a taste for astronomy, and made those discoveries which will ever be memorable in the science. Henry died in 1784, and John then became heir to his grandfather, Sir John, but died only two years subsequently, so that on Sir John's death in 1789 the baronetcy and estates fell to Henry's second son, also named Henry, and born, like his brother, at Groningen. The title became extinct on the death of the eighth baronet in 1839. The estates had, in the meantime passed, through the will of Sir Harry, son of the last Sir Henry and seventh baronet, to another family, that of Holyoake, who took the additional name of Goodricke. The author of the work from which I am quoting tells us that he had often heard his grandfather, William Goodricke of Dearham, of a collateral branch of the same family, speak of the scenes of hospitality he had witnessed, when a boy, at the house of Sir John Goodricke, grandfather of the astronomer. He states that there is a portrait of the latter at Gilling Castle, near Richmond, in the North Riding of Yorkshire, of which I am sure we should all be glad to see a copy, if it could be made and photographed.

Yours faithfully,

Blackheath, 1902, June 2. W. T. Lynn.

The Flash-spectrum.

Gentlemen,—

If I may be permitted to make a few further remarks in reference to Mr. Fowler's interesting communication in your last number, I should like to say that my own results are in substantial agreement with those of Mr. Mitchell in the tables of selected lines which Mr. Fowler gives.

These show clearly the tendency of certain enhanced lines to appear as strong lines in the flash-spectrum, and I may admit further that a considerable number of the stronger enhanced lines of both iron and titanium coincide with abnormally strong lines of single origin in the flash. This is proved by a comparison of the intensities estimated by four different observers from the results obtained at the eclipses of 1898, 1900, and 1901.

It is possible I may have under-estimated the importance of these coincidences, which are doubtless of great interest and significance *per se*. However, the question at issue is whether or not there is such a fundamental difference between flash- and Fraunhofer spectra as to lead us to abandon the view that the flash-layer is the seat of the Fraunhofer absorption-lines.

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* It is interesting to remember that this is the town of the last Gold Medallist of the Royal Astronomical Society.

If an extended comparison is made over a long range of spectrum of the intensities of isolated lines of single origin it is found that for each element there is a close general correspondence with the intensities of the dark lines. The agreement is by no means exact in detail, since the enhanced lines in many cases produce abnormal intensities. I give as an example a table showing the results I have obtained with the titanium lines between λ 3500 and λ 5000.

Titanium Lines in Sun and Flash, λ 3500 to λ 5000.

All lines in Rowland's table assigned to Ti only excepting those which are obscured in the flash by strong hydrogen lines.

Intensity in ⊙.	No. of lines in ⊙.	No. of lines in flash.	Average intensity of flash-lines.
1	38	0	0
2	27	4	10 *
3	27	9	8
4	19	13	16
5	7	7	21
6 and over	5	5	48

* The average intensity of the four flash-lines is increased by two enhanced lines at λλ 3505·06 and 3520·40; omitting these, the average is 1.

It is from this general correspondence of intensity that I have been led to conclude that the flash really represents a portion of the absorbing stratum, viz., the upper more diffused region.

The lower region immediately in contact with the photosphere may give emission-spectra in which the enhanced lines are less prominent; and although this stratum is not able appreciably to impress the photographic plate at eclipses, it may yet have a very considerable absorptive effect, perhaps as much or more than the much deeper upper region. It appears to me, however, that there is no ground for abandoning the view that the *whole depth* of the flash-layer is effective in producing the absorption-lines; and in the case of the hydrogen and calcium lines the whole depth of the chromosphere itself is effective. This is indicated by the narrow absorption-lines at the centres of H and K, which agree exactly in width and absolute intensity with the corresponding bright lines of the chromosphere.

Yours faithfully,

J. EVERSHED.

Kenley, Surrey,
1902, June 21.

The Eclipse of Ammianus.

Gentlemen,—

If it were clearly established that our lunar tables gave us accurate results when applied to eclipses which occurred 1500 years ago, we might fall back on some such hypotheses respecting that of Ammianus as those mentioned by Mr. Lynn. But where is the proof of this accuracy? What eclipse of that antiquity is recorded to have taken place on the day and at the place given by the tables?

I may perhaps mention that a cycle of 521 years (of $365\frac{1}{4}$ days each) appears to answer well for solar eclipses pretty nearly in the same locality. Thus, on June 16, A.D. 885, there was a total eclipse in Scotland, mentioned in the *Chronicon Scotorum*. On June 16, A.D. 1406, there was an eclipse nearly total at London, and on June 29, A.D. 1927 (the same date in the New Style), we shall have another eclipse nearly total in London. But as regards this one of Ammianus, the return on Aug. 28, A.D. 881, seems only to have been recorded in France. This seems to confirm the suggestion that the tables place the eclipse of Ammianus too far east. I do not, of course, imply that the cycle which I propose is a perpetual one, or that the eclipse is visible in precisely the same district on each return; but it seems to answer recorded eclipses with considerable accuracy. Truly yours,

Dublin, 1902, June 9. W. H. S. Monck.

OBSERVATORIES.

Royal Observatory, Greenwich.—Visitation Day, on June 7, like most of the days in the first part of the month, was wet, and the invited visitors were not so numerous as in former years. The Astronomer Royal was compelled by indisposition to keep within doors, but was able to present his Report to the Board of Visitors in person.

Transit-Circle.—The number of transits observed during the year was 11,133, of circle observations 9666. These are completely reduced to 1902 April 8, and the observations to May 4 are nearly so. The apparent correction for discordance between observations of the nadir and stars observed by reflexion was found to be $-0''{\cdot}29$, which is very nearly the mean value of this quantity during the last ten years. The correction for the R—D discordance during 1901 was found to be $+0''{\cdot}062+0''{\cdot}442$ sin z.d.: it is noted that the coefficient of sin z.d. has gradually increased from $0''{\cdot}1$ in 1898 to its present amount. The correction to the tabular obliquity of the ecliptic was found from observations in 1901 to be $+0''{\cdot}20$, and the mean error of the Moon's place (after Newcomb's corrections had been applied to Hansen's tables) was $-0^{s}{\cdot}136$ in R.A. and $-0''{\cdot}59$ in N.P.D. deduced from 117 observations.

The Altazimuth.—This instrument has been used in four positions in the meridian as a reversible transit instrument, and also on some occasions in the prime vertical and other azimuths for observation of the Moon and Planets. 2366 observations of R.A., 1976 of N.P.D. of the Sun, planets, and stars were made during the year, and besides these the observations of the Moon numbered 46 and 47 in the two elements respectively.

Equatorials.—16 disappearances and 4 re-appearances of stars occulted by the Moon have been observed. The observation of double stars has been continued with the 28-inch. The 26-inch refractor has been used to take photographs of Neptune and its satellite, the results of which have been already communicated to the Royal Astronomical Society. With the reflector long-exposure photographs of Nova Persei have been taken, including six with exposure of 4 hours or longer, all of which clearly show the nebulosity around the star.

The Astrographic Chart and Catalogue is progressing satisfactorily. All the plates but one have now been taken, and such plates as are considered inferior to the general standard are being replaced by other plates of the same fields. The measures of half the zone (*i. e.*, from N. Dec. 64° to 72°) are printed, and about three-quarters of the measures of the whole zone have been made. The reproduction of the chart-plates on an enlarged scale has been begun.

A special paragraph of the Report relates to the Reflex Zenith Tube, whose character has lately been rehabilitated by Dr. Chandler. This instrument has undergone some slight alterations, so that it is possible now to observe with it stars which are fainter than γ Draconis and at a greater distance from the zenith.

The number of photographs in the series of sun-pictures for the year 1901 is 359; but on 80 per cent. of these the Sun was free from spots.

Reference is made in the Report to the work done by the Greenwich observers at Sumatra and Mauritius on the occasion of the Total Solar Eclipse on 1901 May 18, and to the re-determination of the difference of longitude Paris—Greenwich, and the usual information is given about the meteorology, time-distribution, and chronometer trials during the year.

Kodaikanal and Madras.—The Report of Prof. Michie Smith for the nine months April to December 1901 includes the work done at both these observatories, of which he is Director; but the work done at the Madras Observatory is reported to him by Prof. R. Ll. Jones, who is Deputy-Director of that institution.

The building of the new observatory at Kodaikanal has been practically completed during the year, but some modifications of the original plans have been found necessary in order to protect the interior more thoroughly from the effects of strong winds, which when accompanied by rain make the rooms damp. The construction of the magnetic building took some time, owing to the difficulty in blasting out the rock for the underground room.

The principal instruments of the observatory at present are the Cooke equatorial of 6 inches aperture, the Lerebour and Secretan equatorial of 6 inches aperture, and a Dallmeyer photoheliograph similar to that in use at Greenwich, none of which are new. A transit-instrument and chronograph, both of which belonged to the Indian Survey, a mean time clock, a sidereal clock, spectroscopes, micrometers, and several other smaller instruments.

A plan of work has been laid down for the Kodaikanal Observatory by the Observatories Committee of the Royal Society as follows:—

(1) That the most widened lines in the sun-spots should be visually observed daily, six of such lines being observed between F and *b* and six between *b* and D.

(2) That other widened lines should be noted.

(3) That visual observations should be made of the prominences and chromosphere.

(4) That photographs should be taken by the Hale-Deslandres method.

(5) That photographs should be taken of sun-spot spectra, for which no other comparison spectrum is necessary but the solar spectrum; that meteorological observations are to be made; and that actinometry, seismology, and cloud photography are to be pursued if these three last are possible, as well as the earlier part of the programme.

This scheme of work has been followed out as far as possible, but it was seriously interfered with by building operations, and there were few sun-spots to be observed. Mr. Michie Smith says that the past year has been one of distinct progress, but a great part of the work done has been more or less experimental. Much time has been devoted to the adjustment of instruments, the supervision of workmen, and the training of assistants. The year 1901 seems to have been abnormally rainy and stormy, and generally trying to the health of the staff.

The work at the Madras Observatory now is simply that of the time service and meteorological records. During the year, 222 transits of clock-stars were observed. The Fort time-signal gun failed on 19 occasions out of 550. The rainfall for the year was 59·84 inches, being 10·82 above the average. A storm which occurred at the end of the first week of December seems remarkable. It was a depression of only slight intensity, but gave somewhat stormy weather over the Bay and on the Madras coast. Its chief feature was the abnormally heavy rain, for on, December 9, 10·62 inches fell at the observatory, a fall which has only been exceeded once during the last 41 years, viz. on 1877, May 18, when the fall was 13·01 inches.

UNIVERSITY OBSERVATORY, OXFORD.—The principal feature of the Savilian Professor's Report is the rapid progress which is being made at Oxford with the measurements of the plates for the Astrographic Chart. Of the 1180 plates to be measured and reduced at Oxford, 930 were completed by April 30, 1902, the output for the year being 149. At this rate the measurement and reduction will be easily completed in two years more. Prof. Turner states that he has the question of publication in mind, but has not yet settled in what form he will give the measures. It is extremely important to print the measures in as small bulk as possible, and to exclude everything which will not be of real utility. As an instance, the advisability of giving references to such stars as are in the *Bonn Durchmusterung* is considered, and Prof. Turner thinks the utility of these references will hardly compensate for the extra bulk of the volume. It seems to us that the B.D. designation of stars has been so generally accepted and adopted—*e. g.*, in the Astronomische Gesellschaft Catalogues and the Harvard Photometric Catalogues,—that they will be found a very great convenience, and save endless pieces of mental arithmetic for those who wish to identify stars in the Astrographic Catalogue.

A list is given in the Report of a number of investigations connected with astronomical photography undertaken by the Professor and his staff during the year, and published in the *Monthly Notices*. The Director also pays a tribute to the zealous work of Mr. Bellamy, who has the superintendence of the regular work of the observatory.

Reference is also made to the need for a residence at the Observatory. We have already expressed our view that this addition would add to the efficiency of the institution.

WINDSOR, NEW SOUTH WALES.—Mr. Tebbutt's report for the year 1901 compels our admiration, as its predecessors have done; and that this feeling is shared by his fellow countrymen may be inferred from some remarks made by Mr. R. L. J. Ellery to a certain Australian scientific association, which run as follows:—

> In 1854 Mr. John Tebbutt, of Windsor, New South Wales, following the footsteps of Sir Thomas Brisbane, at Parramatta, began an astronomical career, and has since built and equipped an excellent observatory, with good instruments, and has devoted his time, money, and energy to the advancement of astronomical knowledge in Australia, in a thoroughly earnest and practical manner, and some of the results of his earliest work were contributed to the Sydney press as early as 1856, and perhaps earlier. Subsequently, we find valuable contributions, both observational and mathematical, in many of the most important astronomical publications of Europe and America, and since 1887 he has published privately each year an account of his work. His splendid cometic work, extending over nearly forty years, is in itself a grand contribution to astronomy, independent of the results he has secured from observations embracing a wide range of subjects. To Mr. Tebbutt's enterprise,

persevering and well-directed personal work, belongs no small share of Australia's contribution to astronomy during the last thirty years. He was the first discoverer of the grand comet of May 1861, which has since been known as Tebbutt's Comet.

It will be seen from this that Mr. Tebbutt is getting an old man, and each year, in his Report, he threatens that advancing years will compel him to forego some of his observing work; but the record now in hand in no way shows signs of this, for during the year 1901 local time was determined on 106 nights, and for this purpose 588 stars were observed with the transit instrument. In the extra-meridian department 273 micrometric measures were obtained of the great comet on 32 evenings, extending from May 3 to June 13. The planets Venus, Ceres, Parthenope, Melpomene, and Diana were observed, and among the remaining observations may be mentioned the measures of 28 pairs of the most interesting double stars.

NOTES.

The Toronto Astronomical Society.—The volume of the 'Transactions' of this Association for the year 1901, with which we have lately been favoured, seems larger than that of previous years, and in its new cover looks more imposing. Judging from the inside, it seems that the Society, the membership of which is now something over a hundred, aims rather at treating astronomy from the philosophical and educational side rather than from the observational, as the evening meetings, which are reported, have generally, as their *pièce de résistance*, an address or lecture on some branch of astronomy, such as the Sun and allied phenomena, the wandering of Mars, Halley's Comet, or some similar subject, and these lectures are all very excellent. Observation, however, is not altogether neglected, for Mr. A. F. Miller contributes a good series of observations of the brightness and colour as well as of the spectrum of Nova Persei throughout the year, and there are other contributions of this kind. Some notes on mirage observations are interesting. A well-authenticated case is quoted of a mirage being seen of a town 160 miles distant, and mention is made of the Silent City of Alaska, which is the name given to a mirage frequently seen on the Alaskan coast, and thought by sea-faring men to resemble the city of Bristol, England.

At the meeting of the Society on June 11 M. Loewy, of the Paris Observatory, was elected an Honorary Fellow, and on the same occasion the name of M. Camille Flammarion was suggested for the same honour. The juxtaposition of the two names suggested some apt remarks contrasting these representatives of two well-known types of astronomers :—

> To those who know these eminent French astronomers only by their writings and work no greater contrast seems possible than appears to exist between them. They would be typical champions of the aims of the two parties in our Society if a contest were being waged between what may be called its right and left wings. Some, feeling the enjoyment and profit of knowing even the elements of astronomy, and moved by altruistic impulses, would make it the main object of a body like ours to popularize the science, to bring under Urania's influence, if only for a time, all who wish to know a little about the heavenly bodies, their telescopic appearance, their general movements, and their composition. Others wishing to carry the study to a point beyond the standard of ordinary illustrative lectures, and feeling that a Society cannot rise above the grade of its average membership, are careless about its numerical strength, and would prefer the adhesion of a few real students to the casual membership procured under temporary excitement. M. Flanmarion would lead the *vulgarisateurs.* He would argue truly that without a warm public interest in scientific thinkers and workers, and especially in patient observers, such men would lack the support and appreciation which are indispensable. M. Loewy, while not disagreeing with such a view, would be content that others should discharge this duty and allow him to pursue in peace and quietness the work he loves.

We regret to read of the death of Mr. Thomas Lindsay, the Secretary of the Society, with whom we ourselves have had some pleasant correspondence, especially anent his History of the Nautical Almanac, which has been mentioned in these pages, and which he has unhappily left unfinished. Mr. J. R. Collins now holds his position as Secretary.

NUMBER OF STARS PHOTOGRAPHED WITH DIFFERENT EXPOSURES. —The work on the Astrographic Chart and Catalogue at Greenwich is beginning to furnish some interesting information as to the number of stars of different photographic magnitudes in the sky. In the Astronomer Royal's Report it is stated that the stars on the Chart plates which cover a belt five degrees of declination wide (*i. e.*, between declination +65° and +70°) have been counted, and the numbers have been compared with the corresponding numbers given by the Catalogue plates and in Argelander's catalogues. The resulting ratios are as follows:—If the number of 9th-magnitude stars in Argelander is taken as unity, the number of stars shown on the photographs with 20 seconds exposure is 3·5, and the number shown with 6 minutes exposure is 15·4, whilst the number shown on the Chart plates to which an exposure of 40 minutes is given, will be represented by 73·0.

Somewhat remarkable are the figures which show the number of stars on the Catalogue plates in the individual zones, one degree broad. These numbers increase as the declination increases, whilst naturally the number of square degrees in each zone diminishes, so that the number of stars per square degree in the different zones are:—

Between Dec. +65°	and +66°	62·0
+66	„ +67	66·0
+67	„ +68	69·5
+68	„ +69	75·3
+69	„ +70	85·3

THE OXFORD OBSERVATORIES.—A somewhat curious incident is mentioned in Prof. Turner's Report. Sir Henry Acland bequeathed, by means of a codicil to his will, various small articles to the Chancellor, Masters, and Scholars of the University of Oxford, and specified the places in the University where he would like them kept. Among them was the astronomical clock used by Dawes when he made his early equatoreal observations, which the codicil said was to be kept in the University Observatory. But the Executors deposited the clock, not at the University Observatory, but at the Radcliffe, for one of them, Mr. H. D. Acland, son of Sir Henry, said that he knew his father intended this to go to that observatory, and that an error had been made in writing the codicil. The Savilian Professor naturally waived his claim to the clock, but points out the anomaly of a clock which belongs to the Chancellor &c. of the University being placed in a building over which they have no control.

THE DEFICIENCY OF RAINFALL: VARIABLE WEATHER IN JUNE. —In the seven years ending the 31st December, 1901, the deficiency of rainfall, as compared with the fifty years' average, amounted to 23·70 inches, or very nearly an average year's rainfall. This deficiency was still further swelled by the deficient rainfall in the four months January to April of the present year to the extent of 3·39 inches, bringing up the total deficiency to 27·09 inches to April 30. The rainfalls in May and June having been somewhat copious, an idea appears to have arisen in the popular mind that much has been done to clear off this deficiency; but as the aggregate amount in these months does not exceed 6½ inches, or 2½ inches above the average, it is obvious that very little effect has yet been produced in reducing the total, which now stands slightly below 25 inches.

The month of June has exhibited very varied conditions. It was generally cold and wet till the 22nd, the mean temperature for these days being 54°·5, or 4°·2 below the average. On the 9th, 10th, and 14th the mean daily temperatures were 48°·5, 48°·2, and 48°·9 respectively, all being exceptionally low, and nearly 10° below average values. After the 22nd the weather became very fine and warm, and the mean temperature of the remaining days of the month exceeded the average by 4°·9, the temperature reaching 80° on three days.

The list of holders of the Order of Merit, newly created by the King for men eminently distinguished in their respective paths in life, contains the name of an astronomer, Sir William Huggins, to whom we offer congratulations. Lord Kelvin and Lord Rayleigh are also in this list; and as it consists at present of only twelve names, science may be considered to be well represented.

We regret to have been in error last month in stating that the Meeting of the British Astronomical Association would take place on June 25. Owing to the proposed ceremony of the Coronation, this Meeting was postponed until July 9. The Royal Astronomical Society will not meet again until November next.

The French observers, M. Bigourdan and M. Lancelin, have finished the first stage of their work in the re-determination of the longitude Paris—Greenwich.

The Degree of Doctor of Science *honoris causâ* was conferred on the Astronomer Royal by the University of Oxford on June 24.

From an Oxford Note-Book.

Lord Rayleigh, Lord Kelvin, Lord Lister, and Sir William Huggins: Science is nobly represented by these four names in the twelve constituting the new Order of Merit. While, however, the Sovereign of the Order lies at the point of death we cannot rejoice as we should. May he live to add many more names to the illustrious list which appears to-day! (June 26).

Two extracts from Evelyn's Diary, which I came across the other day, may be new to some readers:—

> 1676, Sept. 10. Dined with Mr. Flamsted, the learned astrologer and mathematician, whom his Majesty had established in the new Observatory in Greenwich Park, furnished with the choicest instruments. An honest, sincere man.

We know that this is not wholly accurate: for Flamsteed had to buy his own instruments. Nevertheless we will accept the last four words, which depend upon personal observation. The other extract is not from the diary proper, but from a letter to Doctor Beale, 27 August, 1668:

> You may haply call to remembrance a passage of the Jesuit Honorati Fabri, who speaking of perspectives, observes, that an object looked on through a small hole appears magnified: from whence he suggests, the casting of two plates neatly perforated, and fitted to look through, as preferable to glasses, whose refractions injure the sight.

Can anyone throw any light on the meaning of this passage? or is it simply the mistake of a man who theorized without trying his suggestion practically? I have tried two small holes in front of the eyes in various ways, but failed to obtain any advantage whatever: as one would naturally expect.

A RATHER elaborate joke appeared in Oxford recently in the shape of an ostensible new review, with the title "Mind!" and edited by A. Troglodyte. Only one number has yet been published, and as the price was 4*s*., perhaps the joke is considered to have gone far enough. The following extract from page 3 concerns us to some extent and will serve as a specimen of the general style:—

> It is the fault also of the subject if we have not always overcome the proverbial difficulty of not writing satire, and at all events we may claim that there was not an ounce of malice in our fun. As to this, the subjoined report of the famous geloiologist, Dr. Joe King, Ph.D., F.R.S., F.G.S., F.O.S., etc., bears gratifying testimony:—"The jocoscopic analysis of the light of the Nova Mentis, 1901, shows a pretty continuous bright spectrum, chiefly composed of the 'enhanced' lines due to the presence of large quantities of the most frivolous gases. This is an indication of extremely genial warmth, which is confirmed by the fact that even the most refractory solids appear to have been volatilised, and to shine by their own light. The most delicate instruments, however, failed to show any dark lines (even in poetry) due to malice. The cause therefore of the phenomenon remains obscure. It would be premature to decide whether it arose from the spontaneous combustion of a hitherto unobserved philosophic luminary or was due to a collision of academic wits. Nevertheless, I feel it my duty to state that I consider it a *very grave* phenomenon, as indicating serious and incalculable convulsions in the unintelligible world and perhaps heralding the end of an epoch in philosophic history."

THERE is something very pleasing about an apt parallel, as poets know; and the final words of the following paragraph satisfy the requisite conditions admirably. Who wrote them one can only guess; for they come from an unsigned obituary note in *The Eagle*, a magazine conducted by the members of St. John's College, Cambridge, which has produced many good things before now. The writer of the notice is obliged to confess that the subject of it was not a good lecturer:—

> It was all very different from the precise, well-ordered tuition of others: the wistful air of the Lecturer anxious to communicate his learning, yet lacking the power to do so; the puzzledom of his victims struggling with an unfamiliar subject and the added difficulty of a foreign tongue (*i. e.* the best German or French book on the subject). The knowledge we felt was there if we could only get at it: the instructor was anxious to impart it—it was as if the stoppers of the decanters were inexorably jammed, and host and guests alike disappointed.

Perhaps a further extract may be given from the same admirable notice. The writer goes on to say something generally about good and bad lecturers, and continues as follows:—

> The capacity for lucid oral exposition is certainly not given to all possessed of high mathematical powers. Mr. Pendlebury's shortcomings as a teacher were shared in those days by two men of quite different stamp and greater genius. The late Professor Cayley used to pour out algebra to a select and sorely tried audience. He wrote it out as he went along, not on a blackboard but on sheets of paper at the same table with his class, and therefore upside down as far as the class was concerned. He had apparently no idea whether his listeners were following him. One lost link and the rest of the lecture was an arid waste. Professor Clerk Maxwell, with his curious hesitation of manner, and haunted by a perpetual indecision, was in his way even more vexatious. First, suggesting that the temperature of a heated bar at a given point should be denoted by x; rejecting this on second thoughts "because it is too good a letter to waste"; discarding m "because you will be sure to mix it up with n"; finally, with a happy smile, adopting t as a useful alternative and then—using all the letters indiscriminately and apologising for each in turn.

A RECENT visit to Durham enables me to report that the Almucantar is working beautifully. There is some indication of a small change, during the night, in the inclination to the vertical at which the instrument floats: which has not yet been traced to its source. It may not be possible to localize this change, which is very small and gradual, but quite sensible: sometimes changes in collimation, of which this is a case, cannot be localized. But this will not seriously matter. The observations indicate the rate of change with such accuracy that proper allowance can be made for it; and the way in which the errors of particular star-places are reproduced time after time is remarkable testimony to the success of the instrument. There ought to be many more of them. Assuming, for the moment, that they only give the errors of fundamental star-places in a new way, the check is a most valuable one, since declinations, for instance, are given independently of division-errors, flexure, R—D, and a variable refraction. And those who have seen the results of the observations, as I have, would claim increase of accuracy for the instrument in addition to re-arrangement of systematic error. At the present moment no better move could be made for fundamental astronomy than to set up an Almucantar in the Southern Hemisphere.

THE Annual Visitation of the Royal Observatory, Greenwich, on June 7, though overshadowed by the illness of the Astronomer Royal and the wretchedness of the weather (which consisted of heavy showers), nevertheless attracted a considerable gathering;

and a fair number assembled to dine afterwards under the presidency of Sir George Stokes (neither the President of the Royal Society nor the President of the Royal Astronomical Society being available). An incident of the evening was the recital, to music, of some verses which contain astronomical allusions: and by the kindness of the reciter I am here enabled to give the full text. (One of the verses has appeared in these notes before.) He tells me that the music was composed by the late Sir Arthur Sullivan for his opera "Trial by Jury."

Since I was young and called to the stars,
There have been a number of changes;
'Tis more than five-and-fifty years
Over which my memory ranges.
The Visitors once didn't drink French 'pop'
In a restaurant of exquisite finish;
They dined at four, on beer and a chop,
At the 'Crown and Sceptre' in Greenwich.

In Airy's early days, I ween,
Assistants were not many;
Computers few and far between,
Indeed there were scarcely any.
And so they had to work the more,
And "Transit Circle Duty"
Meant all the hours of the twenty-four,
Which wore out youth and beauty.

They used to find the distance of the Sun
By observing Transits of Venus;
Now we choose from the planets one
That doesn't come between us.
And Sir David Gill declares he's able,
By using a heliometer,
To beat the seven-figure logarithm table
And any known arithmometer.

In olden times, and modern too,
We have tried to find how far is
The initial meridian kept at Home
From that famous city Paris.
The answer we hope to get some day—
The only remaining bar is
That Paris from Greenwich seems further away
Than Greenwich is from Paris.

I've seen a lot of improvements made
In compass and chronometer,
And we've found what a principal part is played
By the Front Court Ext. Thermometer.
And soon in charting his place at sea
A navigator won't call that a log
Which takes no account of the R—D,
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EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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Vol. XXV. AUGUST, 1902. No. 321.

THE BRITISH ASTRONOMICAL ASSOCIATION.

W. H. Wesley, *Vice-President*, in the Chair.

Secretaries: J. G. Petrie and
A. C. D. Crommelin.

The concluding Meeting of the twelfth Session of the British Astronomical Association was held at Sion College, on Wednesday, July 9. The names of five Candidates for Membership were read and passed for suspension. The balloting-list for Officers and Members of Connoil for the ensuing Session, prepared by the present Council, was read. No additional nominations were made by the Meeting.

An *interim* report of the Solar Section was presented by the *Rev. A. L. Cortie* (the Director). Observations made on 352 days during the year 1901 were recorded. The total number of groups of sun-spots which appeared was only 24, or 20 less than in 1900. Only two groups attained a disc-area of over 1·0 (the unit being $\frac{1}{5000}$th of the visible disc)—that which appeared first on May 19, and after a second appearance was last seen on June 26, and that which crossed the disc between the dates Nov. 13–24. The former attained a maximum disc-area of 7·6 units on May 26. Not only was it a large spot for the minimum epoch, but it appeared on the day after the total solar eclipse visible in Sumatra and was coincident in position with an unusually disturbed area observed in the corona on the Sun's east limb, and also with a fine prominence, thus showing an intimate connection between the various phenomena. There was no magnetic storm of even moderate violence connected with the spot during its first appearance, when it attained its maximum area and went through rapid changes, though, if magnetic storms and sun-spots were related as effect and efficient cause, some response of the magnets ought to have followed this, the only great solar storm of an otherwise exceptionally quiet year.

Mr. Adams asked if it would be possible to fill in the missing dates from some other source, and thus render the returns complete.

Mr. Maunder said he had had the pleasure of hearing Father Cortie read his paper at the Royal Astronomical Society on the great spot of May last year, in which he brought out many interesting facts; but he (the speaker) had been rather surprised that Father Cortie placed so much stress upon the fact that there was no magnetic disturbance coincident with this particular solar outburst, because although the spot of May 19 last year was a very remarkable outburst for that time of the solar cycle, it was but a small group as compared with the great groups which they had at maximum. There could be no doubt whatever as to the general correspondence between the sun-spot curve and the variations in the magnetic diurnal range. Year after year the two varied together, and not only so, but some of the minor irregularities of the one curve were also reproduced in the other. But when they came to isolated outbursts, it was only the very greatest solar disturbances which found a distinct and evident response in the behaviour of the magnets. In the period over which the Greenwich observations ran—from 1874 to the present time (28 years)—if the half-dozen biggest spots of the period were picked out it would be found that they corresponded exactly in time with the half-dozen greatest magnetic disturbances, and it seemed to him that a correspondence of that kind could not be accidental.

Capt. Noble said: It must be a source of gratification to every one to hear the impartial account which Mr. Maunder had given on the alleged connection between sun-spots and magnetic storms, because in the particular case under notice the group of spots was visible to the naked eye, and there was no magnetic response whatever.

Mr. Crommelin said, with regard to completing the record for every day in the year, he thought that would not be very easy to accomplish. The Royal Observatory had photographic records from three observatories, two besides its own, viz.—Dehra Dun, in India, and Mauritius,—and even then there were generally about eight days in the year missing. But he thought the missing of a few days in the year could not appreciably affect the character of the results. It was not likely that the few missing days had anything very different from the rest of the year.

The Chairman in thanking Father Cortie on behalf of the Meeting for his report, said the Director was to be congratulated upon having secured observations on every day of the year except thirteen.

Mr. Maunder exhibited and explained two drawings which had been prepared from two of Mrs. Maunder's photographs of the eclipse of May last year. The first shown had, he said, been prepared from a short-exposure photograph taken at the commencement of totality. The prominence to which allusion had already been made could be seen very distinctly, and just above it the very marked disturbance in the corona which took place just above the great spot which came into view on May 19. The second drawing was prepared from a photograph taken about mid-totality, and exposed for a much longer time, viz. 8 seconds—the other photograph having had an exposure of ¼ second. In this case the great streamer in the N.E. portion of the limb was seen very clearly to be made up of interlacing streamers. There was also a very curious interruption of the North Polar rays. The centre of it he could not determine, but it was seen distinctly on three or four different photographs, so that of its objective existence there could be no doubt whatever.

Replying to the Chairman, Mr. Maunder said the diameter of the Sun's image on the original photographs from which the drawings were made was about ⅔ inch. The photographs were taken with the aid of Mr. Newbegin's telescope, with a 6-feet focus and 4¾ inches aperture.

The Chairman remarked that the matter was a very interesting one. This appeared to be the first clear instance of a distinct connection between a sun-spot and a coronal disturbance. The corona of 1901 was of quite the minimum type. Mr. Maunder had pointed out a curious interruption of the polar rays—a singular streak which ran obliquely across them. It was probably on a different plane from the polar rays, and was, perhaps, part of the mass of rays which formed the great extensions east and west of the pole.

The Rev. C. D. P. Davies read a paper on "The Coelostat," based for the most part, as he explained, upon practical experience with a little model of his own construction. Mr. Davies also presented the Association with a 12-inch mirror which he had ground and polished during the past winter.

The Chairman thanked Mr. Davies for his handsome present.

A paper on "The Trapezium in Orion," contributed by *Mr. Hugh Wright* (N.S.W. Branch), was read, dealing with the supposed variability of the fifth and sixth stars. From observations he had made, the writer was convinced of the variability of the latter star. Many, he said, believed both to be variable. He urged Members to carefully study the Trapezium when it should be next favourably situated, and to record all their observations of the visibility, estimates of magnitude, and colour of the fifth and sixth stars.

Capt. Noble said that a good many years ago he paid a great deal of attention to the Trapezium of Orion. Ordinarily the fifth star in the Trapezium was easily visible with the $4\frac{2}{10}$-inch aperture of his equatorial, but for one whole apparition of Orion that star was hardly discernible at all, whereas the sixth sprang into comparative prominence just for that year.

Mr. J. W. L. Child said he also had paid a good deal of attention to the Trapezium, and the sixth star always seemed rather faint—he estimated it to be about 12·4 mag., while the fifth was about 11½.

Capt. Noble. In one apparition?

Mr. Child. Yes.

Capt. Noble. I have had the same experience, but this was perfectly abnormal.

Mr. Maw said he had experienced a good deal of difficulty in seeing the sixth star sometimes with a moderate aperture. He had never seen it more prominent than the fifth, but he had observed the Trapezium on nights when he thought he ought to see the sixth quite easily, judging from other stars, and had not been able to do so. The fifth had seemed normally bright, but the sixth had been quite invisible.

The Rev. T. E. R. Phillips said he had always found the sixth star an exceedingly difficult object to observe with his 9-inch reflector.

Mr. A. B. Cobham (N.S.W. Branch) contributed a paper on "Some Southern Stars, probably Variable," in which he urged systematic work on the part of Members of the Variable Star Section.

The Rev. T. E. R. Phillips (the new Director of the Jupiter Section) drew the attention of the Members to several matters of special interest and importance in connection with the present appearance of the planet.

Messier's Nebulæ.

[Continued from p. 269.]

M. 25. $18^h\ 24^m{\cdot}1$, S. 19° 2′.—Smyth describes it as "a loose cluster of large and small stars in the Galaxy." Closely south following this nebula is the short period variable star U Sagittarii, which varies from 7·0 to 8·3 with a period of about 6¾ days. It was discovered by Schmidt in 1866.

M. 26. $18^h\ 39^m{\cdot}7$, S. 9° 29′.—A little south following the star 2 Aquilæ in "Sobieski's Shield." Sir John Herschel describes it as large and pretty rich; stars 12th to 15th magnitude. Smyth calls it a "small and coarse, but bright cluster of stars"; and this is confirmed by a photograph taken by Dr. Roberts in August 1892.

M. 27. $19^h\ 55^m{\cdot}3$, N. 22° 27′.—The famous "Dumb Bell" nebula. It lies a little south of the star 14 Vulpeculæ. It has been drawn by Sir John Herschel, d'Arrest, Lord Rosse, and Lassell. At one time Lord Rosse thought that it might possibly be resolvable into stars, but Huggins found a gaseous spectrum. Photographs by Dr. Roberts and Dr. Wilson show that it is really a globular mass surrounded by a broad and darker ring which gives it the dumbbell appearance.

M. 28. $18^h\ 18^m{\cdot}4$, S. $24^\circ\ 55'$.—A globular cluster. It lies about 1° north preceding the star λ Sagittarii. Messier describes it as a nebula without a star, but Sir William Herschel resolved it into stars, and Sir John Herschel describes it as "very bright; round; very much compressed . . . . resolved into stars, 14 . . . 16 m.; a fine object." This nebula with Messier's numbers 8, 16, 17, 18, 20, 21, 22, 23, 24, and 25 all lie in a comparatively small area of the sky surrounding the 4th magnitude star μ Sagittarii.

M. 29. $20^h\ 20^m{\cdot}5$, N. $38^\circ\ 11'$.—About 2° south of γ Cygni. Smyth describes it as "A neat but small cluster of stars."

M. 30. $21^h\ 34^m{\cdot}7$, S. $23^\circ\ 38'$.—Closely north preceding the 5th-magnitude star 41 Capricorni. There is an 8th-magnitude star close to it. It was resolved into stars by Sir William Herschel in 1783, and Sir John Herschel describes it as "a globular cluster; bright, 4′ long by 3′ broad; all resolved into stars 16 m., besides a few 12 m." Lord Rosse found a spiral arrangement of the stars.

M. 31. $0^h\ 37^m{\cdot}3$, N. $40^\circ\ 43'$.—The great nebula in Andromeda. Plainly visible to the naked eye, a little preceding the star ν Andromedæ, and quite a conspicuous object even in a binocular field-glass. Al-Sufi refers to it as a familiar object in his day (tenth century). This magnificent nebula has been frequently drawn, and has been so often described in astronomical books that a detailed account is unnecessary here. Several photographs by Dr. Roberts show its real character. At first it was supposed to be annular, but Dr. Roberts says, "That the nebula is a left-hand spiral, and not annular as I at first supposed, cannot now be questioned; for the convolutions can be traced up to the nucleus, which resembles a small bright star at the centre of the dense surrounding nebulosity; but, notwithstanding its density, the divisions between the convolutions are plainly visible on negatives which have had a proper degree of exposure"; and he thinks that "these photographs throw a strong light on the probable truth of the *Nebular Hypothesis*, for they show what appears to be the progressive evolution of a gigantic stellar system." He finds its apparent diameter about 2⅓ degrees. This great nebula seems, however, to be not gaseous, as Huggins finds a continuous spectrum. A small "new" or "temporary" star suddenly appeared near the nucleus in August 1885. It had faded to the 16th magnitude in Feb. 1890.

M. 32. $0^h\ 37^m$, N. $40^\circ\ 18'$.—A small bright round nebula a little south of the nucleus of the great nebula in Andromeda (M. 31). It was discovered by Le Gentil in 1749. It is said to have been resolved into stars by Lord Rosse's 3-feet telescope. Its spectrum is similar to that of the great nebula.

M. 33. $1^h\ 28^m{\cdot}2$, N. $30^\circ\ 9'$.—Between β Andromedæ and α Arietis, nearer to β Andromedæ. It was described by Sir John Herschel as a remarkable object, extremely large, round, and very rich and resolvable into stars; but a photograph taken by Dr. Roberts, in Nov. 1895, shows it to be really a spiral nebula. There is a nucleus of "dense nebulosity" with about 20 stars involved, and the other parts of the nebula contain hundreds of faint nebulous stars of about 16th or 17th magnitude. It is of considerable apparent size, measuring about 1° long by ½° width.

M. 34. $2^h\ 35^m\!\cdot\!6$, N. 42° 21'.—About 5° north preceding Algol (β Persei). Just visible to the naked eye. Messier called it "a mass of small stars." Sir John Herschel described it as bright, very large and but little compressed, with scattered stars of 9th magnitude. Smyth says "it is a scattered but elegant group of stars from the 8th to the 13th degree of brightness on a dark ground, and several of them form coarse pairs." A photograph taken by Dr. Roberts in December 1892 shows a loose cluster of stars down to the 15th magnitude; but it is not very rich, and the stars can be easily counted. On the print the densest part of the cluster does not apparently contain more than 100 stars.

M. 35. $6^h\ 2^m\!\cdot\!7$, N. 24° 21'.—A little north preceding the star η Geminorum. Just visible to the naked eye. Sir John Herschel described it as a very large, rich cluster with stars 9th to 16th magnitude. Lord Rosse called it "magnificent," and he counted 300 stars in a field of 26 minutes of arc, or less than the Moon's apparent diameter. Lassell described it as "a marvellously striking object." On a photograph taken in Feb. 1893 by Dr. Roberts he counted 620 stars in a field of 26 minutes. But a glance at the photograph shows that it is not nearly so rich in stars as the globular clusters.

M. 36. $5^h\ 29^m\!\cdot\!7$, N. 34° 4'.—About 2° following the star ϕ Aurigæ. Sir John Herschel describes it as bright, very large, and very rich, with stars of the 9th and 11th magnitudes. Lord Rosse called it a coarse cluster. Smyth says: "a splendid cluster .... a rich though open splash of stars from the 8th to the 14th magnitudes, with numerous outliers." But these descriptions are somewhat misleading, as in a photograph taken by Dr. Roberts in Feb. 1893 the stars seem comparatively few in number, and do not much exceed 100, at least on the print. Compared with the following cluster—M. 37 in the same constellation—M. 36 is comparatively poor.

M. 37. $5^h\ 45^m\!\cdot\!7$, N. 32° 31'.—About midway between ϕ and κ Aurigæ. Sir John Herschel describes it as a rich cluster with large and small stars. Smyth calls it "a magnificent object, the whole field being strewed as it were with sparkling gold-dust; and the group is resolvable into about 500 stars from the 10th to the 14th magnitude, besides the outliers." This description is confirmed by a photograph taken by Dr. Roberts in Feb. 1893, in which the stars are shown down to about the 16th magnitude. The surrounding region is pretty rich in stars.

M. 38. $5^h\ 22^m\!\cdot\!0$, N. 35° 45'.—About 1½° north of ϕ Aurigæ. Smyth describes it as "A rich cluster of minute stars . . . It is an oblique cross with a pair of large stars in each arm, and a conspicuous single one in the centre." Webb says "Glorious neighbourhood."

M. 39. 21^{h} 29^{m}, N. 47° 59′.—About 3° south preceding π' Cygni. Smyth describes it as "A loose cluster, or rather splashy galaxy field of stars, in a very rich vicinity."

M. 41. 6^{h} $42^{m}{\cdot}7$, S. 20° 38′.—About 4° south of Sirius, just visible to the naked eye. It is referred to by Aristotle in his 'Meteorologies' as a star "with a tail." Messier described it as a mass of small stars. Smyth called it "a scattered cluster . . . divided into five groups"; and Webb says "Superb group . . . Larger stars in curves with ruddy star near centre," which Espin suspects to be variable in light.

M. 42. 5^{h} $30^{m}{\cdot}4$, S. 5° 27′.—This is the great nebula in Orion, which has been so fully described, drawn, and photographed that a detailed account is unnecessary here.

M. 43.—This is the small nebula closely north of the great nebula in Orion, M. 42.

M. 44. 8^{h} $34^{m}{\cdot}3$, N. 20° 20′.—This is the Præsepe of the old astronomers in the constellation Cancer. A scattered cluster to the naked eye. 36 stars were counted in it by Galileo, but of course it contains many more.

M. 46. 7^{h} $37^{m}{\cdot}2$, S. 14° 35′.—A little preceding the star 2 Puppis (Argo). Sir John Herschel called it "a superb cluster of stars, 12 . . . 16 m." It includes a planetary nebula (Herschel), which Lassell, Lord Rosse, and Dr. Roberts found to be annular. Smyth describes it as "A noble though rather loose assemblage of stars from the 8th to the 13th magnitude."

M. 47. 7^{h} $50^{m}{\cdot}2$, S. 15° 9′.—This lies about 3½° following the preceding, M. 46. Sir John Herschel says: "Place from Wollaston's Cat."

M. 49. 12^{h} $24^{m}{\cdot}7$, N. 8° 33′.—It lies to the west of δ and ϵ Virginis and forms an equilateral triangle with those stars. Smyth calls it "A bright, round, and well-defined nebula." Webb says, "Faint haze in beautiful position between two 6 mag. stars."

M. 50. 6^{h} $58^{m}{\cdot}1$, S. 8° 12′.—Between Sirius and Procyon and about 4° south of the star 19 Monocerotis. Sir John Herschel described it as a remarkable cluster, very large and rich, with stars 12th to 16th magnitude. Smyth says it "is an irregularly round and very rich mass," and Webb calls it a "brilliant cluster." A photograph by Dr. Roberts taken in March 1893 shows that it is not very rich. On his print the stars may be easily counted and probably do not exceed 200 in the main body of the cluster.

M. 51. 13^{h} $25^{m}{\cdot}7$, N. 47° 43′.—This is the wonderful spiral nebula in Canes Venatici. It lies about 3 degrees south preceding η Ursæ Majoris. Sir J. Herschel described it as a double nebula, the larger with a nucleus and ring round it. Its spiral character was discovered by Lord Rosse, and his drawing agrees well in general outlines with photographs taken in April 1889 and

May 1896 by Dr. Roberts, who finds "both nuclei of the nebula to be stellar, surrounded by dense nebulosity, and the convolutions of the spiral in this as in other spiral nebulæ are broken up into star-like condensations with nebulosity around them."

M. 52. $23^{h}\ 19^{m}\cdot 8$, N. 61° 3′.—About 1° south of the star 4 Cassiopeiæ. Described by Sir John Herschel as large, rich, round, and much compressed, with stars 9th to 13th magnitude. Admiral Smyth saw it of a triangular form and resembling "a bird with outspread wings," and adds that "the field is one of singular beauty under a moderate magnification." Lord Rosse thought it contained about 200 stars. A photograph taken by Dr. Roberts in Aug. 1892 confirms the description given by previous observers.

M. 53. $13^{h}\ 8^{m}\cdot 0$, N. 18° 42′.—In Coma Berenices a little north preceding the star α. It was described by Sir John Herschel as a bright globular cluster, very compressed in the centre, and stars of the 12th magnitude. Smyth calls it "a brilliant mass of minute stars from the 11th to the 15th magnitude, and from thence to gleams of star-dust with stragglers." A photograph by Dr. Roberts in April 1892 shows it to be "an aggregation of small stars close together," and involved in nebulosity.

M. 54. $18^{h}\ 48^{m}\cdot 4$, S. 30° 39′.—About 2 degrees preceding ζ Sagittarii. Sir John Herschel describes it as "a globular cluster; bright; round; gradually brighter in the middle; 2½ diam.; resolved into stars 15 m., with a few outliers 14 m."

M. 55. $19^{h}\ 32^{m}$, S. 31° 13′.—Sir John Herschel describes it as "globular; a fine, large, round cluster; 6′ diameter; all clearly resolved into stars, 11, 12, 13 m." Observing with a 3-inch refractor in the Punjab sky the present writer saw glimpses of stars in it with power 40; will not bear high powers with this aperture; very much like but not so bright as the great cluster in Hercules (M. 13).

M. 56. $19^{h}\ 12^{m}\cdot 27$, N. 30° 0′.—Between β Cygni and γ Lyræ, nearer the former. A globular cluster in a rich region. Sir John Herschel says resolved into stars of 11–14 magnitude. A photograph by Dr. Roberts in August 1892 shows it to be a globular cluster with rays of stars projecting from it.

M. 57. $18^{h}\ 49^{m}\cdot 9$, N. 32° 54′.—This is the well-known "annular nebula" between β and γ Lyræ. Drawings by Sir John Herschel and Lord Rosse agree well with photographs taken by Dr. Roberts between 1887 and 1895, but it is more elliptical in shape than the drawings show. The central opening is not quite black, but is filled in with faint nebulous light. A faint star in the centre is suspected of variability. Lord Rosse, Secchi, and Chacornac thought this nebula might be resolvable into stars, but Huggins finds a gaseous spectrum.

M. 58. $12^{h}\ 32^{m}\cdot 6$, N. 12° 22′.—In the nebulous region in Virgo. Sir John Herschel says, "bright; large; irregularly round; very much brighter in middle; resolvable."

M. 59. $12^{h}\ 37^{m}$, N. 12° 13′.—A little north of ρ Virginis.

Smyth calls it "a bright little nebula." It is in the same low-power field with M. 60.

M. 60. $12^h\ 38^m{\cdot}6$, N. 12° 6′.—A little north of ρ Virginis and in a low-power field with M. 59. Smyth describes it as "A double nebula . . . about 2′ or 3′ from centre to centre, the preceding one being extremely faint." There is another small nebula near.

M. 61. $12^h\ 16^m{\cdot}8$, N. 5° 2′.—A little north of the star 16 Virginis. Smyth describes it as "A large pale-white nebula, but so feeble as to excite surprise that Messier detected it with his 3½-foot telescope in 1779." Webb says, "Faint; bright centre." Lord Rosse found it spiral. A photograph by Dr. Roberts in May 1899 shows it to be "a right-hand spiral." *

M. 62. $16^h\ 54^m{\cdot}8$, S. 29° 56′.—About 6 degrees following and a little south of the star τ Scorpii. Messier described it as "resembling a little comet." Sir W. Herschel resolved it into stars and described it as a miniature of M. 3. Sir John Herschel says: "Globular cluster, bright; large; round; superb; about 7′ diameter; all resolved into stars 15 mag., very equal. The most condensed part is a perfect blaze, but not quite in the centre."

M. 63. $13^h\ 11^m{\cdot}3$, N. 42° 34′. A little north of the star 20 Canum Venaticorum. Sir John Herschel describes it as very bright and large with a bright nucleus. A photograph by Dr. Roberts taken in May 1896, with an exposure of 2 hours and 25 minutes, shows it to be a spiral nebula, "with a bright stellar nucleus in the centre of dense nebulosity." Huggins finds a continuous spectrum.

J. E. Gore.

[To be continued.]

The large Fireball of Sunday, 1902, *July* 13.

A magnificent meteor was seen from many places in the south of England on July 13 last at $10^h\ 30^m$ P.M., and several observers have privately sent me their descriptions of the phenomenon. The newspapers have also contained many references to the meteor, and I am much indebted to Mr. W. H. S. Monck, of Dublin, for a large number of interesting cuttings. Mr. W. E. Besley, of Clapham, S.W., has kindly supplied some MS. accounts, including a careful observation by himself. Altogether about 60 descriptions have been received, though the majority have proved of little scientific value, as they simply allude to the sudden and startling brightness of the object and indicate, only in a very general way, the quarter of the sky in which it was presented. The meteor passed over the Straits of Dover, and the atmosphere being very clear over England, it was well observed, and particularly so from London and the suburbs, as it descended in the E.S.E. near the stars of Delphinus. The Moon (just past her first quarter) was shining in the

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* See *Knowledge*, August 1901.

S.W. and must have detracted from the otherwise brilliant nature of the event; but in spite of this circumstance the meteor was a splendid object, giving a dazzling flash as it fell, and apparently rending the sky with its short and sharp trajectory.

From some of the descriptions the following extracts have been made:—

F. Leigh Martineau, Hammersmith, W.—Everything was suddenly and brilliantly illuminated by an intensely bright meteor. The ball of light forming its head was of the same size as the Moon and it left a tail of reddish colour about 3 degrees in length which endured for 10 seconds.

C. H. Caffyn, Hornsey, N.—Brightness a little stronger than ordinary street arc lamp. Moved quickly; from start to burst occupied 3 sec. Path from about 316° +12° to 313° ±0°.

W. E. Besley, Clapham, S.W.—Flash exceeded the brightest moonlight. Path 311° +21° to 310½ +16½°. Left a streak, most of which disappeared in 30 sec., but a condensation of it remained faintly visible for 3 mins.

E. Holmes, Hornsey Rise, N.—Brilliant meteor began just S. of γ Delphini and pursued a sinuous course of 5°. Moved swiftly. Red at first, it rapidly changed to yellow, green, and blue, then to brilliant whiteness as if a magnesium shell had exploded. Track remained visible for 2 mins. and a luminous patch for some minutes longer. Light superior to that of the full Moon.

E. Rabone, Highgate, N.—The proximity of the diamond of stars in Delphinus enabled the place where the meteor burst to be fixed with tolerable accuracy. This was at 312½° +11°. With regard to its track, the only track that he saw was between the point named and about 3° N. of it.

W. H. Maw, Outwood, Surrey.—Brilliancy equal to full Moon. Began at 295° +33° and burst at 310° +25°. Directed from Vega. Width of the glowing mass about 1°. Duration of the flash about 3 secs. The trail remained visible for 15 or 16 seconds and two condensations in it for 37 seconds.

T. Crumplen, Hungerford Road, N.—Meteor lit up the sky for an instant with a strong bluish light aud left a serpentine trail extending over 10°. The end of the flight was at 305° +17° and the direction was from ζ Cassiopeiæ.

H. W., Ealing, W.—Meteoric flash lit up the sky like noonday and immediately afterwards a fiery trail of a curious sinuous form and reddish in colour was seen at an elevation of 45° or 50° in the S.E. This faded away in 20 secs. No sound was heard.

G. C. Grover, Fulham, S.W.—Vivid light caused the observer to glance upwards, when he saw what appeared to be a magnificent green or blue rocket burst at a point about 10° S.E. of the zenith. It moved about 10° to the south in an almost perpendicular direction, leaving a sinuous trail of fire about 10° long, which remained visible about 1 min.

F. T. Wheler, St. Albans.—Terrific flash illuminated sky. The light came from a ball of red and green colour travelling from N. to S. Left a trail which continued to glow with a soft yellow light for 30 secs.

H. E. P., Clapham Park.—Startled by a brilliant, cold blue light like that of vivid lightning, but lasting some seconds longer. S. of the zenith there was seen a bright serpent of fire apparently composed of a chain of closely united stars. This continued to burn in the sky for 15 secs., then parted into two and disappeared.

O. H., Stamford Hill, N.—Brilliant flash, illuminating everything like daylight, from a magnificent meteor high in the S. sky. Left a trail.

H. Johnson, London, N. W.—Trail like a string of fire-balloons tapering similar to the tail of a kite.

M. L. Rouse, Beckenham, Kent.—A silvery meteor, one-fourth as large as the Moon, gave a bright gleam of daylight. Left a glowing streak just south of the zenith. Burst and left a second curving streak to the S. The first streak was short, about six times the length of the meteor's head, and remained in sight for 30 secs.

W. Chaplin, Flaxman Rd., S.E.—Brilliant flash caused observer to look upwards, when he saw a kind of incandescent wavy ribbon, about 2 inches wide, which remained visible 15 seconds and appeared to be not more than 20 to 30 feet above him.

E. W. Barlow, Surbiton, Surrey.—Descended from a point preceding 5 Pegasi and vanished near the cluster 15 M Pegasi. As bright as the Moon in her quarters. Fell almost vertically. Duration 1 second. Vivid streak of red and green remained visible 1 minute.

E. K. B., Croydon.—Appeared almost immediately overhead, simultaneously with the church clock chiming the half-hour, and burst with a bluish light illuminating the whole country round for a second or two. Left a brilliant trail in a direction from N.W. to S.E.

A. C. S., Croydon.—After a sudden and dazzling flash the observer in looking upwards beheld a large and curved luminous streak in a S.E. direction.

R. Shute-Leete, Claygate, Surrey.—Brilliant meteor appeared a little S. of E., exhibiting many dazzling and lovely colours, illuminated the sky. Left a tail of some length remaining stationary for a minute.

Lieut.-Col. Tupman, Harrow.—Brilliant meteor passed behind chimney in azimuth S. 55° E. and altitude 33°.

J. B. C., Three Bridges, Sussex.—Meteor gave a startling illumination and left a large patch of scintillating light, exactly like the discharge from an enormous rocket.

H. Junor, Brighton.—Like an electric searchlight. Left a remarkable trail.

Rev. J. P. Monnington, Eastbourne.—A blaze of blue-coloured light flashed out and then a chain of light was seen in the N.E.

and high up in the sky. Another observer at Eastbourne says the trail lasted 30 secs. and seemed to take the form of a silver band.

Samuel Jones, Bury St. Edmunds.—Dazzling flash from a brilliant meteor, which at its maximum was almost due S. below and to the right of Altair and about as far from Altair as that star is from β Cygni.

A. J. Barton, Small Heath, Birmingham.—Brilliant meteor, like a huge sea-rocket, appeared in S.E., alt. 55°, and travelling in a slightly downward direction, burst at an elevation of 40°. Emitted a train of bright golden sparks. The head swelled out into a ball of vivid bluish light, finally exploding with a terrific flash, the effect being similar to, though on a much more gigantic scale than, that produced by the use of magnesium light for the taking of a photograph. Duration 1½ second; length of course 15°.

F. J. Jervis-Smith, Oxford.—Intensely brilliant meteor in E.S.E., falling almost vertically. Length 15°, altitude 45°.

W. F. Denning, Bristol.—Meteor gave a brilliant prolonged flash, though the Moon (8½ days old) was shining. Left a short, glowing, vertical streak from 329½° +12° to 332° +8°. This was watched for 30 seconds, when thin white clouds veiled the streak as well as the surrounding stars.

At *Slough* the meteor is said to have burst like a huge and brilliant rocket. At *Addiscombe* near *Croydon* it was described as a ball of fire as large as the Moon. At *Torquay* it brilliantly illumined the bay and scattered thousands of small stars in its wake. In *Essex* it was seen as a ball of blue fire leaving a serpentine trail of sparks, which lasted about half a minute. At *Sandy, Beds*, it appeared like a bright star suddenly bursting into flame. At *Cambridge* the trail lasted 50 secs. Some of the spectators allude to the first flash as giving the appearance of a rent in the sky revealing a furnace of flame behind.

At *Usk* in *South Wales*, *Mr. W. J. Nicholas* says that when the meteor burst "it sent out a shower of rays." At *Aberdare*, also in *South Wales* (and about 205 miles distant from the observed end-point of the meteor's flight), *Mr. J. T. Davies* states that "it flashed like lightning through the sky and burst into a thousand yellow stars, which presented a magnificent spectacle." At *Moretonhampstead, Devon*, *Mr. W. J. Stephens* describes the object as being low down in the eastern sky and travelling in a northerly direction. Moretonhampstead is about 210 miles distant from the place of the meteor's final disruption. An observer at *Aintree*, N. of *Liverpool*, was 245 miles from the meteor; but the latter observation is somewhat doubtful, since the object is said to have been in the south and at an altitude of 50°, whereas it should have been in the S.E. and between 17° and 10° high. It was conspicuously seen at *Northampton*, *Shrewsbury*, *Cambridge*, *Ramsgate*, *Kings Lynn*, *Hunstanton*, and many other places; in fact, the brilliant apparition appears to have attracted notice from observers in every town in the southern half of England.

Not many of the descriptions are sufficiently explicit and accurate to be utilized in deriving the real path of the meteor. There seems little doubt, however, that the radiant-point was at about 315° +31° near ζ Cygni and that the meteor crossed the Straits of Dover from above a point near Boulogne to a point near Dungeness on the coast of Kent, descending from 89 to 51 miles during a luminous course of 51 miles. The velocity is somewhat doubtful, but it appears to have been approximately 26 miles per second. The earth-point is indicated very near Uckfield, Sussex, and it would be interesting to get observations from this locality, where the meteor must probably have been seen as a stationary ball of fire near the star ζ Cygni. It appears to have been comparatively isolated, for though the writer at Bristol watched the sky on 9 nights, between July 6 and 16, no other meteors were recorded from the same radiant. The shower was, however, seen last year, and it has been previously recognized on several occasions as under:—

June 26–July 11, 1872......	315° +32°	15↙s	W. F. D. (From Italian observations.)
July 4–Aug. 22, 1848–70.	315 +31	...	Greg & Herschel.
July 5–30	317 +32	...	Schmidt.
July 15–20, 1901	317 +31	6	W. F. D.
July 19–29, 1848–72	315 +31	22	Heis.

The fireball seems to have had a shorter course and a swifter motion than that usually exhibited. It supplies another example of a brilliant meteoric display occurring on a Sunday evening (see *Observatory*, Oct. 1900). W. F. DENNING.

Bristol, 1902, July 23.

The First Discovery of Nova Persei.

AT a Meeting of the *Société Astronomique* held on June 4, M. Flammarion stated that a youthful member of their own, M. Andreas de Borissiak, a pupil of the classical gymnasium at Kiew, had discovered Nova Persei on February 21, about 8 o'clock in the evening, Poulkoua time, and therefore (allowing for the difference of longitude) about $8^h\ 40^m$ before Dr. Anderson noticed the star at Edinburgh; also that the Astronomical Society of Russia had accorded to M. Borissiak a special medal in honour of this discovery. It may be remembered that it was first mentioned in a letter from Prof. von Glasenapp, printed in No. 3700 of the *Astronomische Nachrichten*. There were, however, two suspicious circumstances with regard to the correctness of the date. In the first place, M. Borissiak stated that the brightness of the Nova was equal to that of Pollux (magnitude 1·2) when first seen, whereas Dr. Anderson noted it as of 2·7 magnitude at 14ʰ 40ᵐ Greenwich time, and it is known that it was considerably brighter next day. In the second place, M. Borissiak's letter announcing the discovery was written on Feb. 24 and received by Prof. von Glasenapp on Feb. 25. Moreover, negative evidence was to hand that the star was not of any remarkable brightness on the evening of Feb. 21; Herr J. Plassmann in particular stating that he had been observing a minimum of Algol at 8ʰ Greenwich time, and must have noticed the Nova had it been then as bright as the third magnitude (*Ast. Nach.* No. 3693). Prof. Kreutz, the Editor, therefore was induced to make some inquiries of Prof. von Glasenapp and Prof. R. Vogel of Kiew, which satisfied him that M. Borissiak's observation was really made at the time stated, but that he overestimated the star's magnitude.

Nevertheless one cannot but feel that grave doubts must still remain on the point, and we would call attention to an article by Herr Leo Brenner, "Ueber den Entdecker der Nova Persei," in vol. iv. No. 36 of the ***Astronomische Rundschau.*** After remarking that he rejoices at the discovery made by so young a man (in appreciation of which the Emperor of Russia had made him a present of a telescope), and hopes that he would be enabled to devote his time to astronomy, yet, in the interests of truth, he cannot help pointing out that the date given was probably erroneous. For proof that it was Feb. 21, although the letter was not sent till Feb. 24, we are told that M. Borissiak bought an astronomical book the next day, the bookseller's receipt for which was dated Feb. 9, corresponding to Feb. 22 by Gregorian style. That week, however, was what is called Butter-week in Russia, the week preceding Lent (Ash Wednesday by Julian style was that year on Feb. 14, corresponding to our Feb. 27), and Friday and Saturday (our Feb. 22 and 23) would be holy days on which no business could be done. This is pointed out in Herr Adolf Richter's *Calendar for Riga*, 1902; and the conclusion drawn by Herr Brenner is that M. Borissiak first noticed the star on the evening of the 22nd (as many others did), or more probably on the 23rd, which would fully explain the brightness it possessed at the time, and which several observers (Herren Hartwig, Plassmann, Schwab) testify it could not have had on the evening of Feb. 21. No one wishes to contest M. Borissiak's independent notice of the star; it is a credit to a young student of only 16, and let us hope, with Herr Brenner, that he will be able to devote his life to astronomy and illustrate it by many discoveries amongst the stars.

We older ones cannot help recalling the discovery of T or Nova Coronæ in 1866, and the attempted claim (disproved by Stone) of a gentleman in Canada to have anticipated Birmingham in the discovery. An Indian officer also claimed to have seen it the day before Birmingham; but he was probably, in view of Schmidt's careful observation of the constellation at the time, a day in error.

W. T. Lynn.

Blackheath, 1902, July 25.

Some interesting Double Stars.

[Continued from p. 270.]

Σ 1883. R.A. $14^h\ 43^m\ 56^s$, N.P.D. 83° 37′, Mags. 7·0 and 7·0.

This is a pair still more resembling in character Σ 1863. The observations from 1832 to 1897 lie along a straight line, but those of 1830 and 1900 fall away in such a manner as to induce a more critical examination of the motion. Projecting the 1900 measures on to the line, we get the annual motion of

0″·009	from 1839	to 1900	0″·013	from 1883	to 1900
0 ·010	„ 42	„	0 ·013	„ 87	„
0 0·11	„ 57	„	0 ·013	„ 97	„
0 ·011	„ 72	„			

which indicates binary motion, and shows that the distances of 1830 and 1890 may be correct. If this is so, then the distance ought to decrease rapidly. The measures are:

1830 ...... 272° and 1″·24
1890 ...... 243 „ 0 ·51

The star is W.B. (11) XIV. 777.

Σ 2402. R.A. $18^h\ 45^m\ 1^s$, N.P.D. 79° 26′, Mags. 80 and 84.

This is a pair which at a first glance seems fixed and uninteresting, but it is worth attention as it may prove to be a binary system with its orbital plane passing through the Sun. Naturally everything depends on the distance-measures. The observations give:

1830	198°	0″·75	1885	205°	1″·10
41	206	0 ·74	95	207	1 ·04
60	211	0 ·80	1900	206	0 ·88
75	205	0 ·93			

The star is Lalande 35038.

Σ 2434. R.A. $18^h\ 57^m\ 36^s$, N.P.D. 90° 51′, Mags. 8·4, 7·9, 10·0.

This is a group of three stars, the star A (8·4) being Piazzi XVIII. 274, the star B (7·9) being Piazzi XVIII. 275.

The Proper Motion of A is	−0″·060	in R.A. and	+0″·20	in N.P.D.
„ „ B	0 ·000	„	+0 ·08	„
giving a relative motion of	−0 ·060	„	+0 ·12	„

If we consider A fixed, then the relative motion of B from the micrometer-measures is

+0·070 in R.A. and −0·11 in N.P.D.,

equal and opposite to that deduced from the consideration of Proper Motions. Hence A and B are independent. The distance is 23″·5.

The Proper Motion of B would separate it from C (if fixed) some 5″ since 1835. The measures of BC are :—

1835	83° and	1″·95
1868	69 „	1 ·50
1900	50 „	0 ·9 ±

clearly showing that C must partake of the Proper Motion of B and that BC is a physical pair. Beyond this the relative motion of B and C is curvilinear and the distance should decrease rapidly. The pair must be followed.

Σ 1989. R.A. $15^{h}\ 45^{m}\ 6^{s}$, N.P.D. 9° 42′, Mags. 7·1 and 8·1.

This star is π^2 Ursæ Minoris, and is very interesting as affording an excellent example of a class of star which often gets neglected, because for a number of years no apparent relative motion has been detected. The orbit happens to be an elongated ellipse and the companion at or near elongation ; measures are made for more or less lengthy periods, and then the pair is left as uninteresting. In the present case the measures from 1832 to 1875 gave little or no indication of motion ; and yet in the subsequent years it has been very rapid and, unfortunately, very few measures have been secured. The positions are practically :—

1834	24°	0″·6	1881	15°	0″·5
41	25	0 ·7	95	358	0 ·2
68	20	0 ·6	1901	220	0 ·1

so that there is a certain amount of evidence that an elongation occurred about 1841. The present interest lies in watching whether the distance will increase, or whether the companion will swing round the focus. Should the latter occur it will strengthen the idea of elongation about 1841, and bring the period under 100 years. Professor Aitken, at Lick, has this pair under notice.

Σ 2021. R.A. $16^{h}\ 8^{m}\ 39^{s}$, N.P.D. 76° 12′, Mags. 6·7 and 6·9.

This star is 49 Serpentis, and has a large proper motion of +0″·157 in R.A., and +0″·416 in N.P.D., which, since 1783, when it was discovered by Sir W. Herschel, would place the companion in the second quadrant at a distance of 50″. It is now in the fourth quadrant at a distance of 4″ only ; consequently it is a physical pair, as both stars must necessarily participate in the large proper motion. When Herschel made measures in 1783 and 1803, he noted the distance as less than 2″, in 1829 it was 3″·4, now it is 4″. There are indications that elongation was reached about 1885, and the distance may be expected to diminish. This pair is then one well worth measuring by possessors of small telescopes. The period is somewhere about 350 years.

Σ 2052. R.A. $16^h\ 24^m\ 30^s$, N.P.D. 71° 23′, Mags. 7·5 and 7·5.

This star is W.B. (2) XVI. 671, and was first observed in 1822, when its distance was about 2″·7. It seems to have increased to about 3″·4, and returned again in 1870 to 2″·7, the change in angle during this period being only 6°. In reality a casual glance at the measures gives the idea that the pair were practically fixed. Fortunately measures were continued which give roughly:—

1823	109°	2″·7
30	110	3 ·0
52	108	3 ·1
65	104	2 ·9
73	103	2 ·6
1883	99°	2″·5
93	96	2 ·0
98	94	1 ·9
1901	93	1 ·8

evidently binary, and we may expect rapid diminution in distance. This is a pair within reach of almost all observers. It has a large proper motion, which, according to Stumpe, is −0″·344 in R.A., and +0″·385 in N.P.D. T. Lewis.

Some Remarks on Almanacs.

Almanacs, in England, are of considerable antiquity. Our Saxon forefathers were accustomed to carve on square pieces of wood the courses of the Moon for a whole year, and those pieces of wood were called by them "Almonaught" (al-moon-heed), whence the word almanac. Almanacs, like books, were also written on parchment, brilliantly illuminated, many of which, of the 14th century, may be seen in the British Museum. The origin of English almanacs was strictly superstitious. Our Saxon ancestors had their prognostics from the Sun and Moon, from thunder and from dreams. One day was favourable for the commencement of business, another to let blood, another was bad for sowing seeds, &c. Some of the Stationers' Company's Almanacs for the first half of the 17th century may still be picked up: the writer has Booker's for 1625, Allstree's for 1632, and a finely bound volume of ten of them for 1640, formerly belonging to Archbishop Juxon (who had been a member of the same Oxford college as himself). My copy of Tanner's for 1662 has the log or diary of the voyage of a coasting-vessel from Newcastle to London written on its spare leaves. A volume of the Stationers' Company's almanacs for 1687, in the writer's possession, comprises eight of them, and one for 1698 fifteen, the names of these being Salmon's, Parker's, Gadbury's, Partridge's, Coley's, The Protestant Almanack, Saunders's, Pond's, Woodward's, Andrews's, Tanner's, Chapman's, Poor Robin's, and two others not published by the Company. Among those in the volume is a copy of Partridge's. This was one of the longest-lived of English almanacs. Partridge was a tailor by trade, born in 1644, died in 1714. He was brought up to the trade of a shoemaker, which he practiced in Covent Garden, but having acquired some knowledge of Latin, astronomy, and astrology, he published an almanac. He acquired a ludicrous immortality by attracting the satire of Swift. The 1698 copy of his almanac states on the titlepage that it was the "ninth of our deliverance from Popery, and the third from the horrid Popish, Jacobite plot," a title which appeared yearly until 1866. It was filled with violent Protestantism, but each day of the Calendar, except Sundays and Saints' days, had appended to it the name of some black-letter Saint, not merely of the English but also of the Roman communion! In its later years Partridge's was a useful almanac. It ceased to be issued after 1871, when its price was 9*d*. About seventy years ago it had risen, like Moore's, to 2*s*. 3*d*., of which 1*s*. 3*d*. comprised the tax impost.

The following, which occurs on the last page of Partridge's for 1758, may have a slight interest about the present time. No Comet had then fulfilled its predicted return, there being none of elliptic orbits known. After stating that it is presumed the present period would be 76 years, and that the particular time of its appearance was uncertain, Partridge remarks:—"What sign it may be expected to begin to appear in: "Jan., scarce to be seen. Feb. ℞ in ♐. March in ♑. April in ♓. May, stat. in ♈. June, dir. in ♉. July in ♊. Aug. end ♊. Sept. stat. in ♊. Oct. ℞ in ♊. Nov. in ♊ and ♉. Dec. in ♉ and ♈."

The almanacs most worthy of note in the 18th century were Weaver's, Parker's, and Wing's. These were somewhat similar, on the whole, useful publications, while others contained very much more astrological matter. The copies I have picked up give a fair ephemeris of Sun, Moon, and planets, eclipses of Jupiter's satellites, good descriptions and diagrams of eclipses of Sun and Moon, descriptions of principal occultations, with an occasional diagram of the latter, also of visible transits of Mercury.

The longest-lived, and formerly the best-known almanac, is undoubtedly that of "Francis Moore, physician." He was born at Bridgnorth, in Salop, in 1656, and practiced in London as astrologer, physician, and schoolmaster. About forty years ago there were several almanacks published with the prefix of Moore's, as Old Moore's, Moore's Almanac improved, Moore's Almanac by Sagittarius, and at least two or three more. Writers have always stated that Moore's Almanac was a continuation of Tanner's, which was supposed to be issued first in 1656 and to have continued till 1698. This must be a mistake, as Partridge's resembled Tanner's far more, and recently I obtained a copy of Tanner's for 1714, so they must have been published side by side.

Francis Moore's Almanac was first issued in 1699. Andrews, of Royston, who died in 1820, was the compiler of it a hundred years ago. He was also astronomical calculator to the Board of Longitude, and for many years corresponded with Maskelyne. Mr. A. Cole, in a communication to *Notes and Queries*, vol. iv. p. 162, states that he had purchased from the son of the said Andrews all his father's manuscripts, consisting of his calculations. In a letter addressed to Mr. Cole, he says:—"My father's calculations for Moore's Almanac continued during a period of forty-three years, and although, through his great talent and management, he increased the sale of that work from 100,000 to 500,000, yet, strange to say, all he received for his services was £25 per annum." The largest impressions of any single book (before the days of penny literature) ever sold are said to have been those of Moore's Almanac.

Some 30, 40, 50, and 60 years ago this Almanac was somewhat unique in its astronomical information. This was in the days of Mr. Woolhouse, of *Nautical Almanac* fame. About half a dozen pages every year were then devoted to full eclipse-descriptions, well-executed diagrams being given of each. This was followed by figures of the principal lunar occultations for each year. When there was a visible solar eclipse, as, *e.g.*, in 1867, 1870, 1873, &c., the times of beginning, greatest phase, ending, and magnitude were worked out and given for 20 or 30 different places in Great Britain. Probably no publication ever issued turned the minds of the young to astronomical studies more than the attractive diagrams and information on such matters given in Francis Moore some years back. Several living astronomers may be said to have had their attention drawn to astronomical pursuits in this way. There was a small amount of astrology in Francis Moore's Almanac until 1872, when it ceased to be inserted. An almanac is still published with a similar outside cover, but otherwise greatly altered; in 1900 astrological matter was inserted again, in 1902 not a figure was given to illustrate either of the visible eclipses, the point that used to be a distinctive feature in this Almanac.

The life of that admirable publication, the *Nautical Almanac*, has not yet reached that of Moore's. My copy for 1769, the third issued, has two treatises bound up with it, the "Perpetual Calculator" and an "Explanation of the Articles in the *Nautical Almanac*," the first of which, at any rate, could hardly have been a portion of the original issue. The eclipse information, even 70 years ago, was, however, somewhat meagre. The total solar eclipse of 1832, about the most important, in point of length, in the whole of the 19th century, is dismissed with a few lines. Francis Moore, for that year, devotes some space to it, describing the track of totality from one continent to another, mentioning also the proximity of Venus to the Sun ($\frac{3}{4}°$), an observation of which has been narrated to me by Mrs. Cheyne, of Croydon, an eye-witness. Moore puts the extreme duration of totality $7^m\ 37^s$, probably too long. Moore's, however, seems to have been the only almanac, at that time of day, that described the track of total eclipses (an abbreviation appearing yearly in Partridge's). About the year 1790, and for some years before and afterwards, a spurious copy of Francis Moore's Almanac was printed at Birmingham. It resembled the true one in a great measure, but small letters were used in the titlepage instead of capitals, and the astronomical diagrams were not so well drawn.

Nor need we pass over, without allusion, the negro, Benjamin Banneker, who published almanacs for some years on the other side the Atlantic. His father was taken as a slave in Africa and sold in America. His mother was the child of natives of Africa. Having had 'Ferguson's Astronomy,' Leadbetter's tables, and some other scientific books lent him, he set to work and diligently studied the nocturnal heavens. He pointed out a mistake in Ferguson's and two in Leadbetter's astronomical tables. Both Messrs. Ferguson and Leadbetter would have looked incredulous had they been informed that their works had been reviewed and corrected by a free negro in the then almost unheard-of valley of the Patapsco. In the year 1792 he took it into his head to begin to publish almanacs. The Baltimore publishers, Messrs. Goddard and Angel, to whom he had been introduced, stated they presented to the public "a complete and accurate ephemeris for the year 1792 calculated by a sable descendant of Africa." The almanac went on to the year 1802, and had a large circulation in Maryland, Virginia, Delaware, and Pennsylvania. The author died in 1809.

A hundred years ago, and since that time the 'Gentleman's Diary,' the 'Lady's Diary,' 'Season's Almanack,' 'White's Ephemeris,' had a considerable circulation in our own country, the proportion of astrological almanacs being actually far less than at the present day.

Melplash Vicarage, Bridport,
1902, May 14.

S. J. Johnson.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Comet III. 1862.

Gentlemen,—

It is now just forty years since the discovery of this comet, which has since obtained additional interest from the fact, first noticed by Schiaparelli, of the great resemblance of the elements of its orbit to those of the Perseid meteors. Perhaps it may be of interest now to recall that its discovery was the first of the many which we owe to Prof. Lewis Swift.

The announcement of this was made by Prof. Hough, then Assistant at the Dudley Observatory, Albany, N.Y. (the late Prof. Mitchel* being Director). After stating that the comet in question had been discovered by Mr. Thomas Simons on the night of the 18th of July, he says* that he subsequently "received a letter from Mr. Swift, an amateur astronomer, at Marathon, N.S., in which he stated he had observed a comet on the 15th and 16th of July, but presuming it to be Comet II. 1862, he gave no public notice of the observation." But, from a diagram afterwards sent, it was found to be an earlier discovery of the comet seen by Mr. Simons, of the Dudley Observatory, on the 18th. On the latter day it was also seen at the Harvard College Observatory (*Ast. Nach.* No. 1419†), where it was followed until the 17th of September. Mr. H. P. Tuttle detected it on the same day (July 18) as Mr. Simons at Albany and Mr. Hall at Harvard College. He says (*Ast. Nach.* No. 1404, vol. lix. col. 187), "The comet first became visible to the naked eye on the 20th of July, at which time it had a strong resemblance in brilliancy to the well-known cluster in Hercules. I estimated that it reached its maximum brilliancy and the tail attained its greatest length on the 2nd of September." On that evening he was able to trace the tail as far as 30° from the nucleus, but beyond 10° from that it was so faint that it would have escaped the notice of any one not accustomed to observations. The last time he saw the comet was on the night of the 17th of September (the same day as that on which it was last seen at Harvard College) after he had entered the U.S. Federal Army and had gone into camp. It was then situated about six degrees from Antares and just visible to the naked eye.

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* He died on the following 30th of October.

Unfortunately there was at first much confusion in numbering this comet, which is sometimes called in the *Ast. Nach.* "Comet II. 1862," whilst, on the other hand, observations of the comet discovered by Bruhns on Dec. 1 are called of "Comet III. 1862," in *Ast. Nach.* No. 1422 (vol. lx. col. 86), though the latter is really the *fourth* comet of that year, having passed perihelion just before the end of it.

The period of the third comet of 1862 (the Perseid-meteor comet) is probably about 125 years in length; it is possible that the second comet of 1737 may be identical with it, but the orbit of the latter, which was observed during only ten days at Pekin by Father Ignatius Kegler, is very uncertain.

Yours faithfully,

Blackheath, 1902, July 3. W. T. LYNN.

The Red Spot Region of Jupiter.

GENTLEMEN,—

I write to call the attention of Jovian observers to a very interesting phenomenon now in progress in the Red Spot region of the planet. It will be remembered that last year a dark spot appeared at the south edge of the south equatorial belt which ultimately extended across the south tropical zone and joined the south temperate belt. Several transits of this object across the central meridian were secured, and a discussion of these by Mr. W. F. Denning (B. A. A. Journal, vol. xii. no. 3, p. 122) showed the spot to be moving at about the normal rate of spots in this latitude. Mr. Denning calculated that the middle of the spot would be in longitude 45° (System II.) on July 17th of this year. Since its first appearance, and especially since the close of last apparition, the spot has exhibited a remarkable longitudinal extension; and observing this region of the planet on the morning of June 19th, I found an enormous area of dark material following the *f* "shoulder" of the Red Spot hollow for more than 35°. There was also a long dark streak connected with the above shading on the south temperate belt immediately south of the Red Spot. Mr. Denning observed the same appearances on the morning of June 21st. Clouds prevented me from seeing this part again until the early morning of June 26th, when I saw close to the *p* "shoulder," or *west* of the Red Spot hollow, a very conspicuous dark spot, extending across the south tropical zone and connected with the south temperate belt. This was also observed on the central meridian by Mr. Denning and myself on June 28th at 3 A.M. ($\lambda=19^{\circ}{\cdot}9$) and 2.57 A.M. ($\lambda=18^{\circ}{\cdot}1$) respectively. There can be little doubt that this dark spot (invisible in its present position on June 19th, though very prominent and obtrusive on June 26th) is the advanced portion of the dark mass above referred to which has now passed the Red Spot and made its appearance west of that object.

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* *Ast. Nach.* No. 1394, vol. lix. col. 29.

† Vol. lx. col. 35.

The interesting question is:—How did it arrive at its present position? Was it diverted to the south by the obstruction of the Red Spot, and, after passing along the south temperate belt and causing the marked intensification of the belt mentioned above, once more strike across the south tropical zone to regain its former latitude, or did it pass *under* or *over* the Red Spot? The latter alternative seems highly improbable, as there is a very brilliant area immediately *p* the Red Spot which appears in no way dimmed by the passage of any dark material. There is still a very large dusky area east of the *f* "shoulder" of the hollow, and it is desirable that its behaviour and motion should be most carefully and critically watched. Some interesting problems may thereby be solved. Cloudy weather has prevented me from obtaining satisfactory observations of this region of late, but the *f* end of the dusky shading appeared to be in mid-transit last night at $12^{h}\ 17^{m}$ ($\lambda=81^{\circ}{\cdot}3$), so that it is now about 21° behind the *f* "shoulder."

Observers will remember that a somewhat similar south tropical spot was in conjunction with the Red Spot in July and August, 1890, and was carefully followed, especially by Mr. A. S. Williams. On that occasion the object on reaching the Red Spot was diverted southwards, extended into a streak on the south temperate belt, and did not return to its former latitude.

May I also call the attention of Jovian observers to the marked acceleration of late in the velocity of the Red Spot? The longitude of the hollow is now distinctly *less* than it was a year ago, so that its motion has become more rapid than that of the zero meridian of System II. (Mr. Crommelin's ephemeris). I have secured the following transits this apparition:—

1902.	Transit time. h	m	λ (System II.). °	
March 25	18	3	44·9	
April 28	16	14	45·9	
May 27	15	11·5	46·1	
June 18	13	17	44·3	
,, 27	15	39	43·4	
July 2	14	45	42·7	
,, 5	12	14·5	42·9	
,, 7	13	53	43·3	
,, 9	13	30	42·7	"Estimated" when past mid-transit.
,, 14	14	40	44·5	
,, 22	12	14	43·2	,, ,,

Yours faithfully,

Croydon, 1902, July 23. **Theodore E. R. Phillips.**

OBSERVATORIES.

Royal Observatory, Cape of Good Hope.—Dr. Gill opens his Report for 1901 with the statement that the object-glass of the telescope presented by Mr. McClean, which had been returned to Sir Howard Grubb for correction, has been definitely accepted, and that this completion of Mr. McClean's munificent gift was marked by the insertion of a stone in the north-west front of the Observatory. The unveiling of this stone has already been described in these pages. The instrument, called the Victoria Telescope, comprises besides the 24-inch object-glass an 18-inch visual telescope, which has been used by Mr. Innes during the year for the measurement of double stars, and with it he has made 342 complete sets of measures on 224 different pairs. He has detected 13 previously unrecorded pairs, of which 8 are of less than 1″ distance, and it is specially mentioned that α Centauri was measured as a double star on 6 days by Gill, and on 27 days by Innes. This telescope has also been used occasionally for the observation of the fainter minima of variable stars, of occultations, and on one night of the comet.

The 24-inch object-glass has been tested, and the separation of the lenses adjusted to correct for spherical aberration and focus. The wave-length for minimum focus remains slightly on the less refrangible side of that of Hγ. The spectroscope was sent to the Cambridge Instrument Company to be fitted with the new prisms which Mr. McClean is providing, unfortunately just before the appearance of the great comet, so that it was impossible to observe that object spectroscopically.

Besides the Victoria Telescope there is another instrument of great interest at the Cape—the new Transit-Circle, which was all received, except the collimators, in April. The building which contains this was described in our abstract of Sir David Gill's report last November. In the present report Sir David gives in some detail the method he proposes to employ to obtain perfectly stable marks to be used as a control on the azimuth of the instrument from hour to hour, and perhaps for very much longer periods. In addition to the ordinary collimators, on the collimator piers there are to be mounted lenses of about 300 feet focal length, and in the principal focus of these there will be marks attached to well-founded piers north and south of the instrument. Difficulty has been experienced in finding a suitable foundation at a short distance below the surface, and if the piers were founded at depths of about 30 feet there would be risk of changes in the piers themselves, and a small angular tilt of the foundation would also cause a large displacement of the lens or mark. Accordingly H.M. Astronomer devised a plan by which the positions of marks attached to the solid rock could be transferred vertically above the ground-level :—

> For this purpose object-glasses of the requisite focal length are to be attached to mountings fixed to the base-rock itself. The lines joining the optical centres of these object-glasses define the lines of Azimuth reference. Means are provided for placing the optical centres of the long-focal lenses and the meridian marks vertically over the optical centres of the four object-glasses which are attached to the bed rock below. The plan employed for this purpose depends on the well-known Bohnenberger method of determining level and nadir. Imagine a pair of intersecting spider-webs fixed to a frame that carries one of the meridian marks (the plane of the wires being horizontal and their intersection vertically under the mark itself). Suppose, further, that these webs are in the principal focus of an object-glass fixed on a suitable stand at the bottom of a shaft which is sunk down to the solid bed rock, and that under this object-glass is a pool of mercury. Then an observer from above-ground would, with a suitable eyepiece and after proper adjustment, be able to see the intersecting spider-webs both directly and by reflection; and, if the mounting of the above-ground mark was provided with suitable adjustments, he would be able to make the reflex and direct images of the intersecting webs to coincide. If, therefore, the intersecting spider-webs are vertically under the mark, the mark may thus be adjusted vertically over the optical centre of the object-glass at the bottom of the shaft. But for practical purposes it is not necessary that the intersecting spider-webs should be vertically under the meridian mark, it is only necessary that the relative positions of the meridian mark and the cross wires should be constant, and this condition is easily secured by attaching both the webs and the mark to the same piece of metal: in that case, when the direct and reflex images of the webs are in coincidence, the mark will occupy a constant azimuth east and west of the optical centre of the referring lens at the bottom of the shaft. Similar remarks of course apply to the long-focus Azimuth lenses.

A contract has been entered into for carrying this into effect.

With the old Transit-Circle about six or seven thousand observations in each element have been made during the year. The Heliometer has undergone cleaning and repair, and in consequence not so many observations have been made with it as usual. The 7-inch Equatorial has been used for the revision of the Cape Photographic Durchmusterung, and the other equatorials for the observation of occultations of stars by the Moon, 97 separate phenomena of which were observed during the year.

The Astrographic Catalogue work is proceeding. 159 plates, containing 100,203 stars, have been measured during the year in both co-ordinates in each of two reversed positions of the plate. The total number of plates measured is now 397, with an average of 555 stars per plate.

The Report gives an account in some detail of the progress of the work on the Geodetic Survey of South Africa. The arc of meridian has now been carried to the Zambesi. The operations of the Anglo-German Boundary Survey in South-west Africa are in steady progress.

CAMBRIDGE UNIVERSITY.—The latest Report of the Observatory Syndicate treats of the work done from 1901 May 25 to 1902 May 24. With the meridian circle 3315 observations were made during the period on 79 clear nights, and 21 which were only partially so. The reduction of these observations, which were chiefly of the zodiacal stars from Sir David Gill's catalogue, is well advanced, and as it appears that only two persons are engaged on the meridian work, the record speaks well for their energy.

The principal work of the Sheepshanks Photographic Equatorial is to be the making of observations for stellar parallax, and a beginning has been made although the weather during the year has been particularly unfavourable. Twenty-four exposures only have been obtained which satisfy the conditions that photographs at opposite epochs of the parallactic displacement six months apart should be taken at the same hour-angles. It is found necessary to impose this condition on account of atmospheric dispersion, although it limits the hours of observation in most cases to the hours immediately following sunset and preceding sunrise, which will greatly diminish the number of photographs obtained.

Two defects have been found and remedied in the instrument: first, the triple photo-visual objective was very sensitive to changes of temperature, and it is necessary in consequence to find the focal setting for each plate by visual observation. Secondly, the electric control of the driving-clock was found to be not very perfect owing to the uncertain action of the relay. A new form of relay presented to the Observatory by Capt. E. T. Dixon, R.H.A., has set this right.

Reference is made in the report to the work of Mr. Hinks on the determination of the solar parallax from observations of Eros, which has already been described in our reports of the R. A. S. Meeting.

The two-prism spectroscope was used on the Newall telescope until December to obtain spectra of nebulæ and faint stars and for observation of Nova Persei. After that date it was dismounted to make room for the four-prism spectroscope returned from Sumatra, which has since been used for the study of the spectra of certain bright stars. Mr. Newall has spent considerable time in studying the material obtained in Sumatra at the Total Eclipse of 1901, May 18.

The north annex referred to in last year's report has been completed and a gas-engine and dynamo installed.

Perth, West Australia.—It is some time since mention was made in this magazine of this colonial observatory, but the annual publications which Mr. Cooke has sent us and we have failed to notice, have been mainly meteorological. His volume of meteorological observations for 1900, which evidently involve a great deal of work, not only due to the observations at Perth, but also in collecting those from outlying stations, is recently to hand, and contains a paragraph summarizing the astronomical position, which is reproduced below in full. We happen to know that Mr. Cooke's lack of assistants has been partially met, and that Mr. G. F. Johns, formerly of Greenwich and of the Cape, is now attached to this observatory.

> The Astronomical buildings are now finished and the instruments in adjustment; but, owing to the want of assistants, I have been compelled to devote most of my time to meteorological work. I am pleased to say that I have at last brought all the arrears of back work up to date. The meteorological observations for various localities throughout the State which have been accumulating since 1875 have now been examined, tabulated, and discussed, and the results will shortly be available in a volume entitled 'The Climate of Western Australia,' to be issued almost immediately. With respect to Astronomy, the Observatory is now pledged to take a share in the great International Photographic Durchmusterung, for which our geographical position, climate, and instruments are eminently suitable. At present the transit-circle is used for the determination of time; and a clock, surmounting a Stevenson Screen containing thermometers, has been erected for the benefit of the public at the principal entrance. This is properly regulated and guaranteed to keep the standard time of the State (or that of the 120th meridian). The clocks at the principal telegraph operating room and railway station in Perth are also regulated by an hourly electric current transmitted automatically by the Standard Clock at the Observatory.

NOTES.

Comet Notes.—Mr. Heber D. Curtis has computed the definitive orbit of Comet 1898 I., and publishes his researches in No. *3* of the *Astron. Abhandlungen* (supplements of the *Ast. Nach.*).

The comet was discovered by Mr. Perrine at the Lick Observatory 1898 March 19. He has also the credit of following it longer than any other observer, his last observation being made on Nov. 15, while no other observer followed it beyond July 18.

The comet was of the 6th magnitude when discovered and of magnitude 16·7 when last seen. Its light at different times was found to vary as $\frac{1}{r^7\Delta^2}$, instead of the usual expression $\frac{1}{r^2\Delta^2}$. When brightest it had a well-marked stellar nucleus, round coma, and short fan-shaped tail, which did not point exactly away from the Sun. Traces of a nucleus were seen even when the comet was of the 16th mag.

Altogether 666 observations are available, made at 34 observatories. The probable error of an observation at each observatory is computed and weights assigned accordingly. In a few cases systematic corrections have been applied to the observations at certain observatories.

All available catalogues have been consulted for improving the places of the comparison stars. Proper motions have been applied wherever there is sufficient material for their determination. The Astr. Gesell. Catalogues are taken as the standard system.

Planetary perturbations have been computed and applied. The greatest is 5″ in R.A., 0″·5 in Dec.

The orbit proves to be an ellipse of comparatively short period. The author proposes to examine in a future paper the original orbit before the comet entered the planetary system.

T	1898 March 17·1308 G.M.T.	
ω	47° 19′ 12″	1898·0
☊	262 26 19	
i	72 31 47	
log q	0·039511	
e	0·980385	
Period	417 years	

Photographic search has been made at Heidelberg for Comet 1895 II. Swift, but without success. A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered on plates taken at Heidelberg by Prof. Wolf and his assistants :—

JA	1902 May 7	JG	1902 June 26
JB	„ May 7	JJ	„ June 26
JD	„ May 11	JK	„ July 6 (prob. Kilia).
JE	„ June 3		
JF	1899 June 9		

JF is a triumph for the stereocomparator, having been discovered with its aid (after previously being overlooked) by Dr. C. Pulfrich.

A planet named JH proved to be identical with JB.

Planet 475, discovered last year at Arequipa by Stewart, has been named Occlo (a queen of the Incas fabled to have been a daughter of the Sun). This planet, whose orbit is more eccentric than any other, will be in N. Dec. 46° next January and will probably be observed again then. A. C. D. C.

OBITUARY.—HERVÉ FAYE. The *doyen* of astronomers of France, and almost of the world, has passed away in the person of M. Faye, who died early in July, aged 88.

Hervé Faye was born on 1814, October 3, and in the year 1836 began his astronomical career, when he was admitted as a pupil at the Paris Observatory, under Arago. Seven years later he was advanced to the position of astronomer, and in the same year, 1843, he was brought prominently before the world by his discovery of the comet which bears his name, and which, moving in an elliptic orbit with a period of about 7½ years, has been observed at every subsequent return. As a practical astronomer he showed his genius by suggesting improvements in instrumental methods. He proposed the zenithal collimator which was adopted at the Paris Observatory, and he was one of the first to suggest the application of photography and electricity to astronomy. A photographic transit-instrument was one of his ideas.

M. Faye's fame, however, will rest rather on his work as a writer and teacher of astronomy than on that just mentioned. He was elected a Member of the Academy in 1847, and devoted himself to physical problems, such as the cause of the shape of comets, meteors, the problems of the solar spots, between which and the movement of our terrestrial atmosphere he drew ingenious analogies. His work 'Sur l'Origine du Monde' reached a third edition.

He occupied a Chair as Professor at the Polytechnic School between 1852 and 1855, and again from 1873 to 1893, when he delivered a course of lectures on Astronomy and Geodesy, which have been published. He was a Member of the Bureau des Longitudes since 1862, and at one time filled the office of Minister of Instruction in the Cabinet. He was elected an Associate of the Royal Astronomical Society in 1848, and has been similarly honoured by societies in many other countries of the world.

BRILLIANT SUNSETS.—Judging from the precedent of the year 1883, when the Krakatoa eruption was followed by some magnificent coloured skies at sunset, many persons have been looking for similar phenomena after the recent volcanic explosions in the West Indies. A correspondent writing to *Nature* from Madeira, under date June 12 stated that on several evenings previously, especially on June 6 and 10, a beautiful pink haze was seen in the west immediately after sunset. Mr. J. E. Clark, writing from Croydon on June 30, gives details of the glow as seen by him at that place on June 24 and from Street in Somerset on other evenings about that date. He compares these appearances with the glows of 1883, and thinks that those of the present year are not nearly equal to the earlier ones in brilliancy. Other peculiar sunset phenomena not quite of the same kind are reported as having been seen about the same dates as above. The following note appeared in *Nature* of July 3 :—

> Dr. C. B. Plowright, of King's Lynn, and some friends with him, observed after sunset a number of rose-red beams of light radiating upward in the western sky, with the sun as the centre. These beams were not of long duration, and changed in position and brilliancy in the course of a few minutes. Sometimes they extended 30° or 40° upwards towards the zenith. On Wednesday, June 25, the beams were brightest about 9 10 P.M. On Friday, 27th, the phenomenon lasted from about 8.50 to 9.10 P.M. After the beams died away, the upper part of the western sky was tinged by a delicate pink haze. Mr. A. R. Jenkin sends a similar account of the beams as seen by him at Trewirgie, Redruth, on June 27, at 8.45 P.M. He noticed at first " a pink glow low down in the south-east, exactly opposite the sun ; this soon sent out streamers. Meanwhile, a pink patch of considerable extent had appeared about midway between the horizon and the zenith in the north-west above the sun, and this also quickly resolved itself into rays converging towards the sun corresponding to those in the east, so that at one time they could almost be traced right through the zenith, and one of the rays lower down to the north could be plainly seen right across from west to east. As the western glow increased so the eastern diminished, until at 9 P.M. there was nothing to be seen in the east, but in the west the rays were most striking. Appearing to rise out of a clear belt of lemon-yellow colour along the horizon, they extended high up into the sky. As the sun got further north below the horizon the rays rotated on their axis (the sun) in the opposite direction. They could be distinctly seen up to 9.20 P.M., when they had almost sunk into the belt on the horizon, which by this time was of a similar pink colour."

The Berlin correspondent of the *Standard* stated on July 7 that for some time past, both at sunrise and towards dusk, there had been visible in that city a remarkable glow of colour on the horizon, the sky exhibiting an appearance of unusual beauty. M. Forel, a well-known observer of certain natural phenomena on the Lake of Geneva, describes in the *Journal Suisse* of July 10 some brilliant sky effects observed by him at Morges on July 5 directly after sunset :—

> A brilliant disc of light of a whitish-yellow colour appeared 30° above the sunset point a quarter of an hour after the sun had set, and lasted for a quarter of an hour. Ten minutes later, a purple circle, sixty degrees in diameter, appeared and sunk lower and lower as the sun increased in distance below the horizon. While this circle was visible the sky was brightened by an afterglow. The red sunset effects observed at Jamaica on May 25, Madeira on June 10, and Bombay about June 25, had not been seen at Morges on July 8 *.

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* *Nature*, July 17.

An apparent Motion of the Universe.—It has been already remarked that the astronomy of the future will deal with the geometry of the Universe rather than with the geometry of the individual solar or planetary systems. Sir David Gill makes a startling contribution to this line of research (*Ast. Nach.* 3800), based on such comparatively humble data as is furnished by a comparison of star-catalogues. In the course of the work of preparation of the Cape Catalogue for 1900 now in hand, it was necessary to compare the Right Ascensions with those of earlier catalogues, and certain anomalous differences were found, which by dividing the stars into groups of R.A., and subdividing these groups into smaller groups of different magnitudes, were found to depend somehow on the magnitude of the stars: *e. g.*, taking the differences of R.A. between Taylor's Madras Catalogue recently reduced by Dr. Downing and the Cape 1900 Catalogue, the results are for

Stars of Mag. 4·0 .... Cape 1900 − Madras $= -0^{s}{\cdot}052$
" " 5·8 .... " " $= -0{\cdot}188$
" " 7·4 .... " " $= -0{\cdot}315$

Now, says the author, these differences cannot be accounted for by a "magnitude" personal equation whose size is of the order of any yet determined. The motion of the solar system through space does, of course, produce parallactic motions in the nearer (*i. e.* brighter) stars relative to the fainter ones, but this effect disappears in the mean, and therefore should not affect the figures now under discussion; therefore we are drawn to the conclusion that the brighter stars rotate with respect to the fainter stars as a whole about some centre, unless Newcomb's proper motions and precessions with which the reductions were made are not to be trusted, and even this latter hypothesis would not account for all the facts observed.

Double Stars.—Herr Bruno Peter, from measures made with the six-inch Repsold heliometer at the Leipzig Observatory, deduces a parallax for σ Draconis of $+0''{\cdot}175+0''{\cdot}010$.

The *Astronomische Nachrichten*, No. 3798–99, contains some double-star measures made at Hong Kong by Dr. Doberck in 1901.

The *Astronomical Journal*, No. 522, contains measures of 70 pairs, mostly Struve stars, made by E. A. Boeger with the 12-inch equatorial of the Washington Observatory.

Napoleon's Comets.—The following paragraph is from the *Globe* of July 12:—"It was a firm legend among the worshippers of Napoleon I. that a comet was visible on the day of his birth in Corsica, and another on the day of his death in St. Helena. Needless to add, deep symbolic significance was attached to these appearances. Baron Larrey, a descendant of the great Napoleonic

Army Surgeon, recently wrote two letters to Faye, the astronomer, with the result that half the legend was destroyed. From these letters, which are published by the *Temps*, the natal star part of the legend is a pure myth, since, although there was a great comet in 1769, it was not visible to the naked eye on August 15, 1769. Even the death star, or comet story, is not more than half true, since the comet could then only be seen with a glass. Like the genius with which it has been associated, the comet was then fast receding from the Earth. Alas for legends when scientists get hold of them!"

CASSINI'S DIVISION IN SATURN'S RINGS.—It will be remembered that Mr. C. T. Whitmell computed that on July 17 Saturn would be in such a position with respect to the Earth that, from some places, it would be possible to see the ball of the planet through Cassini's division *. This prediction has been verified, for Mr. Townshend, President of the Leeds Astronomical Society, states that on July 15 he saw the division as usual, that is throughout the ring and crossing the ball, but that on July 17 the part of the division across the ball was invisible. Mr. Whitmell himself too, on the 17th, with a 9-inch refractor saw the division in the ansæ of the ring, but it was not distinguishable from the ring itself on the portion which crossed the globe.

THE BEN NEVIS OBSERVATORIES.—The meteorological observatories on the summit of Ben Nevis and at Fort William are to be closed at the beginning of next October, owing to lack of funds. Since their establishment in 1883 these institutions have been supported mainly by subscriptions from the public, but supplemented by a comparatively small sum received from the Meteorological Council, which is the only aid they receive from the State. In 1898, when the subscriptions fell off, Mr. J. M. Bernard came forward and generously provided the money necessary to keep the observatories going a little longer, but now the Meteorological Council refuse to continue their grant so that the work on Ben Nevis must necessarily cease.

IN the notice of M. Faye on another page it is stated that he was almost the senior astronomer of the world. Dr. Galle, of Potsdam, and Otto Struve, now of Baden, who were elected Associates of the R. A. S. on the same day as Faye, are both alive. The former was 90 years old on June 12 last.

THE following is the text of the speech delivered by Prof. Love at Oxford in presenting the Astronomer Royal for the Degree of Doctor in Science on June 24:—

Inter mathematicos, qui Cantabrigiæ quattuor et triginta abhinc annos graduati sunt, clarum erat nomen Willelmi Henrici Mahoney Christie, nunc inter omnes omnium gentium astronomos clarissimum. Astronomorum profecto ille annus magno proventu floruit cum in eodem Tripode Georgii Darwin nomen contineatur. Ambo hi viri Collegii Sanctæ Trinitatis socii creati sunt, sed in astrorum scientia alter alteram insistebat viam. Ille solis stellarumque soli parentium ultimam vetustatem investigabat: hic noster se negotio utiliori dedit ut solis stellarum siderumque omnium et locos qui nunc sunt et motus accuratissime notaret. In hoc opere tantam peritiam adeptus est ut iam viginti abhinc annos et Astronomus Regius et Societatis Regalis Sodalis crearetur. Hoc gubernante fere omnia in Observatorio Regio maximo vel novata vel in melius mutata: neque enim id solum curavit ut novis instrumentis cederent vetera, sed ut eadem paullo immutata idonea fierent ad sidera observanda observationesque ita factas memoriæ tradendas quemadmodum iubent astronomi recentiores. Ita vir peritissimus et rem felicissime navavit et ærario publico pepercit. Summa eius in rem publicam merita agnovit Regina nostra Victoria quæ eum titulo Comitis de Balneo ornavit: insigni honore prosecutæ sunt Academiæ Parisensis Petropolitana aliæque complures quæ eum inter externos litterarum commercio sibi adiunctos receperunt. Huius nomine inter Doctores nostros inscripto monstrabit profecto Academia nostra se memorem esse quantum et doctrinæ et hominum utilitatibus Astronomiæ Scientia profuerit.

---

* *Observatory*, June, p. 242.

We regret that a note was published on page 244 of our June number which was not in accordance with facts. Dr. Doberck is not resigning the Directorship of the Hong Kong Observatory, but he wrote to us that he "was going on halfpay on medical certificate," which we interpreted as the note states. But Dr. Doberck is merely at home in Copenhagen on sick-leave and is still the Director of the Colonial Observatory, where he will return when restored to his usual robust health. Whilst in Europe he will continue his double-star measures with larger instruments than are at his disposal in Hong Kong. For the present, Mr. F. G. Figg has been gazetted as Acting-Director of the Hong Kong Observatory.

From an Oxford Note-Book.

Sir David Gill has made a startling suggestion (in *Ast. Nach.* 3800) to the effect that "the brighter fixed stars are apparently rotating as a whole with respect to fainter stars as a whole." Cast in this form the suggestion is probably quite new, though now that it is made we can look back and see how the possibility was contained essentially in previous notions. The idea that the sidereal system is rotating in some way about some axis is by no means new; and if the rotation is not like that of a solid body (in which case all stars would have the same angular velocity), but like that of a whirlpool, the nearer stars would rotate at a different pace from those more remote. If the brighter stars are nearer us we arrive then at Sir David Gill's idea, though there is some reason to lay stress on that "if." But nevertheless the suggestion, put into a concrete form with the evidence of figures to back it up, is so new as to be startling, and we must set to work in the most serious manner to verify the preliminary results given in the paper above cited.

The particular evidence submitted by Sir David Gill is not, it must be confessed, very complete or convincing. It depends on differences of R.A. of three Catalogues, viz. (A) Taylor's Madras Catalogue, as revised by Downing, Epoch 1835; and the Cape Catalogues for 1880·0 and 1900·0 (B and C say): and the differences submitted are the combined effects of "magnitude equation" (the curious variation in personal equation with a star's brightness) and of the possible phenomenon suggested by Sir David Gill. We have, in fact, to show clearly that the differences are *not* due to "magnitude equation" before we can ascribe them to the actual rotation of the stars. Now take first the case of the Catalogues B and C. Sir David Gill writes:—"These results would imply that the magnitude-correction for an 8th-magnitude star as compared with a 4th-magnitude star is $-0^{s}{\cdot}11$, whereas we know with certainty that for the 1880 Catalogne it does not exceed half that amount." Do we know this with certainty? Reasons have been given (see *Mon. Not.* lx. p. 11) for believing that the magnitude-equation at the Cape is under-estimated, though it may be rightly urged that these reasons themselves require modification in the light of this new suggestion. (This point would take too long to consider here.) But, again, were any *measures* of magnitude-equation made so early as 1880 by any method? I fancy such investigations began much later, and it is difficult therefore to be "certain" about such a small quantity.

Taking next the comparison of A and C, "the question at once arises whether the true magnitude-corrections to Taylor's Catalogue are, as would be implied by the above results,

Mag..........	4·0	5·8	7·4
Correction	$0^{s}{\cdot}000$	$-0^{s}{\cdot}136$	$-0^{s}{\cdot}263$

These results are certainly very striking. The difference between 4·0 and 5·8 especially *cannot* be due to magnitude-equation, as Auwers has shown; and the only weakness in the evidence is the small number of stars (21) on which the first number depends. The second pair are not so strong, for Auwers's investigation stops at magnitude 6·0, and there would be nothing unprecedented in a decided magnitude-equation commencing just at this point.

An independent consideration of the evidence given by Sir David Gill, in which he deduces P.M.s from A and C, and applies them to compare B and C, is open to the objection that any errors in A are divided by nearly 3 in the results.

The remarks in the foregoing paragraph are not offered as a serious criticism of the paper, for which I have had neither time nor material; but as an indication of the points on which more information would be welcome when such important interests are at stake; and, it may perhaps be added, an indication that the paper has been read with due attention. Having so read it, and pondered on the reading, I may perhaps venture to repeat the opinion already expressed—that the evidence offered is at any rate such that we cannot afford to rest until this matter is thoroughly sifted. Fortunately photographic measures give us a perfectly simple and easy way of settling it. The comparison of plates taken at epochs sufficiently distant gives the answer directly; and those who have measured plates taken 10 years ago could probably, by repeating the same plates now and comparing the new with the old, get valuable information at once. In his Report for 1901, Sir David Gill mentions the desirability, for other reasons, of measuring the plates taken in the years 1892–96 as well as those taken recently. He has now an additional reason of great weight for so doing.

BY-THE-BYE, in the Report just quoted there is a little slip which will no doubt interest Captain Noble, F.R.A.S., who has always protested vehemently against the use of the same symbol for inches as for seconds of arc. A good example of the confusion which may result is afforded him in the Report of H.M. Astronomer at the Cape of Good Hope for 1901; wherein we learn (on p. 11) that "without making the discovery of double stars a matter of special research, Mr. Innes has detected 13 previously unrecorded pairs, of which 8 are of less than one inch distance"!

A FEW months ago I remarked on the deficiencies of French publications in the matter of dating. Sufficient time has scarcely been afforded for improvement, and if I quote another instance it is merely to emphasize the former complaint and not to make a new one: but the instance is somewhat conspicuous. Four pamphlets have appeared in the usual green paper covers, all entitled "Détermination des Mouvements propres des Étoiles: explication des discordances trouvées dans la Comparaison du Catalogue de Paris avec celui de Lalande, par J. Bossert." They are numbered I., II., III., IV., but there is nowhere any indication of the volume from which they are extracted, and no indication of date or publisher. The paging for each begins at unity. As heading to the left-hand page there is certainly "Catalogue de l'Observatoire de Paris," and at the present moment there is perhaps little difficulty in placing the work, after a little examination of other volumes. But a few additional particulars would have obviated even this necessity: and how about the Librarian of 50 years hence? The work is one of great importance, and may, without straining one's imagination unduly, be supposed in use 50 years hence, or even 100: nor need we strain our imagination to picture the annoyance of the casual user.

It may not be out of place to mention a minor grievance of the same type. Why are so many outside covers of pamphlets inscribed with just the particulars which, when turning them rapidly over, one does not want? I have before me a pamphlet inscribed "Société Hollandaise des Sciences à Harlem. Extrait des Archives Néerlandaises des Sciences Exactes et Naturelles." And that is all! To find out what it is about we have to look inside, and then we learn that it deals with variable stars of long period. To open the first page may not seem a serious matter, but it becomes serious for anyone earnestly at work on many such pamphlets.

Sometimes, though, it might seem better to have nothing at all on the cover, or generalities merely, than a misleading title. This last week I opened an extract from Proc. R. S. vol. 66, labelled "Radio Activity of Uranium by Sir William Crookes, F.R.S.," only to find a paper by an inferior author on the "Brightness of the Corona of 1893."

THE note last month on the magnification of objects "when looked on through a small hole" has produced the following interesting suggestion from Comte de la Baume Pluvinel:—

> May I suggest the following explanation of Honorati Fabri's magnifying hole? Instead of looking at the object through the hole, perhaps he looked at the hole and interposed the object between the eye and the hole. The hole being strongly illuminated, an enlarged shadow of the object is cast on the retina. Looking through two holes improves the image, the shadow being larger. I make the hole in a sheet of paper with a pin, and use the head of a pin as an object.

But the suggestion was that two plates with holes in them could be used like a pair of spectacles, and I don't see how this idea would work at all. It is very ingenious, but I remember that when at school we produced any really brilliant translation of a difficult passage, the stern pedagogue would say "*Ingeniosius quam verius.*"

A NEW book by Prof. C. A. Young is always sure of a welcome, and the newer the book perhaps the greater the welcome. His 'Manual of Astronomy'* recently published is not altogether new, being avowedly an adaptation of other works to particular wants. It "has been prepared in response to a rather pressing demand for a text-book intermediate between the author's 'Elements of Astronomy' and his 'General Astronomy.'" The mere fact of the existence of a need for such a graduated series of works by the same author is a sufficient indication of success. This is not the place for a review of the book; but a word may be ventured in recognition of the paragraphs on Eros and on Nova Persei, which give quite the latest astronomical intelligence available.

---

* 'Manual of Astronomy,' by C. A. Young. Ginn & Co., 1902.

The following extracts from the *Washington Post* of June 24 and 25 seem to be worthy of preservation. Comment is needless; we can only hope they represent journalism and not fact:—

The Naval Observatory.

We do not find any great amount of merit in the suggestions that have been liberated in such vast quantities by the announcement of Capt. Davis' approaching transfer from the Naval Observatory to active sea service. These sugggestions cover a good deal of ground and exploit a great number of arguments and theories, but they all point at last to the proposition that the observatory should be put in charge of a professional astronomer, and that naval officers should be rigorously excluded from any and every participation in its control.

As we say, this idea does not impress us favourably. We see no more occasion for putting the Naval Observatory in charge of a civilian astronomer than for putting any one of our warships in charge of the engineer who actuates it. The observatory is more than a shed with a big telescope inside of it. To have a few scientific gentlemen looking at the planets, stars, and constellations and drawing their own inferences from what they see—this does not constitute the whole object of the establishment or mark the limit of its usefulness. The observatory is a vast concern wherein are employed a large number of men, and at its head must be an official of distinct executive ability. The superintendent must possess the gift of organization and administration. He need not know the Milky Way from the rings of Saturn, but he must know how to command, to plan, and to preserve discipline. Without such a man at the helm, not all the star-gazers in ten States would save the observatory from demoralization, and, since there must be such a chief, where are we more likely to find him than among our navy officers, who are bred to authority in the best of schools, and who, moreover, belong to the corps of which the institution is an asset?

No, no: we take no stock in this cry: "An astronomer for the observatory!" It sounds well, but it will not bear intelligent analysis.

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CONGRESS FROWNS UPON BOARD OF NAVAL OBSERVATORY VISITORS.

Institution will continue in Charge of Naval Officers without any outside Supervision.

Congress has effectively wiped out the chance of civilian supervision of the Naval Observatory by failing to make any appropriation whatever for the salaries of the members of the board of visitors to that institution, authorized by a law passed a year ago. This is accounted in naval circles as a decided victory for the line of the navy.

Through the efforts of Mr. Long when Secretary of the Navy, Congress was induced, despite antagonistic influence, to pass a law directing the inspection of the observatory by a board of five members. This board was appointed by the President and made an inspection, and in its report criticised the conduct of the institution. The observatory is under the immediate supervision of Rear-Admiral R. B. Bradford, chief of the Bureau of Equipment, who was opposed to the interference of any third parties in the affairs of the institution, especially as he was able to show that it did efficient naval work.

Rear-Admiral Bradford, Capt. C. H. Davis, superintendent of the observatory, and other officers have not hesitated to express the view that the board of visitors only created additional expense, and this is also apparently the view of Congress.

There is reason to believe that Secretary Moody will decline to recommend to the President the appointment of a board of visitors when there is no money with which to pay the members for their services.

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T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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Messier's Nebulæ.

[Continued from p. 293.]

M. 64. $12^{h}\ 51^{m}{\cdot}8$, N. 22° 13'.—In Coma Berenices, about 1° north following the star 35. Sir John Herschel described it as very bright and large and thought it resolvable. Lord Rosse found a dark spot on one side. Smyth says "it is magnificent in size and brightness." Webb says, "Magnificent large bright nebula, blazing to a nucleus." A photograph by Dr. Roberts in May 1896 shows it to be a spiral nebula, "with a large bright stellar nucleus; one of the convolutions is very bright, with a dark space between it and the nucleus, which is free from nebulosity—thus producing the effect of contrast between dark and light spaces."

M. 65. $11^{h}\ 13^{m}{\cdot}6$, N. 13° 38'.—A little south preceding the star 73 Leonis (south of θ Leonis). A photograph by Dr. Roberts, taken in Feb. 1894, shows it "to be a left-hand spiral, with the external outline so regularly formed that it resembles an annular nebula with rings encircling it; but the spiral form must be the true interpretation, and the rings of nebulosity with the dark spaces between them and the nebulous star-like condensations together form parts of the convolutions; the dark spaces being the intervals between them."

M. 66. $11^{h}\ 15^{m}$, N. 13° 32'.—In the same low-power field with M. 65. Lord Rosse found it to be spiral, and this is confirmed by a photograph taken in Feb. 1894 by Dr. Roberts, who says: "The photograph shows the nebula to be a spiral, with a well-defined stellar nucleus, which forms the pole of the convolutions in which I have counted fourteen nebulous star-like condensations." Smyth says that at a short distance following M. 65 and M. 66 there is another elliptical-shaped nebula of even larger apparent dimensions.

M. 67. $8^{h}\ 45^{m}{\cdot}8$, N. 12° 11'.—In Cancer, about 2 degrees preceding α. Sir John Herschel described it as a remarkable object, very bright, large, and rich, with stars 10th to 15th magnitude. Smyth calls it a rich but loose cluster. A photograph by Dr. Roberts in Feb. 1891 shows that the cluster is not very rich. It seems to contain about 200 stars as estimated by Sir William Herschel.

M. 68. 12^{h} $34^{m}{\cdot}2$, S. 26° 12′.—In Hydra, about 3½ degrees south of β Corvi, and near a 5½-magnitude star. Sir John Herschel describes it as "A globular cluster, irregularly round; gradually brighter in the middle . . . All clearly resolved into stars 12 m.; very loose and rugged at the borders."

M. 69. 18^{h} $24^{m}{\cdot}5$, S. 32° 25′.—About 2½ degrees north following ϵ Sagittarii. Sir John Herschel described it as "A globular cluster; bright; round; 3′ diam.; all clearly resolved into stars, 14–15 mag. A blaze of stars."

M. 70. 18^{h} $36^{m}{\cdot}3$, S. 32° 25′.—About 2½ degrees following M. 69 (above). Sir John Herschel describes it as "A globular cluster; bright; round; resolved into stars 15 mag."

M. 71. 19^{h} $49^{m}{\cdot}3$, N. 18° 31′.—Between γ and δ Sagittæ. Resolved into stars by Sir William Herschel. Sir John Herschel described it as a very large and very rich cluster, with stars 11–16 magnitude. A photograph by Dr. Roberts in July 1898 shows a cluster in which the "curves and arrangements of stars resemble those in a spiral nebula." The surrounding region is "densely crowded with stars down to about 17th magnitude arranged in remarkable curves and lines which are very suggestive of having been produced by the effects of spiral movements."

M. 72. 20^{h} 48^{m}, S. 12° 54′.—In Capricornus. A globular cluster, discovered by Messier in 1780 as a nebula. Sir William Herschel resolved it into stars with his 20-feet reflector. He called it "a very bright object," and estimated its diameter at 1′ 53″·6.

M. 73. 20^{h} $53^{m}{\cdot}5$, S. 13° 1′.—In Capricornus, a little following M. 72. Sir J. Herschel says, "Cluster ??; extremely poor; very little compressed; no nebula."

M. 74. 1^{h} $31^{m}{\cdot}3$, N. 15° 6′.—A little following η Piscium. Sir John Herschel thought this to be a globular cluster, very large and round, brighter in the middle and partially resolved into stars; but Lord Rosse found it to be a spiral nebula, and a photograph by Dr. Roberts taken in Dec. 1893 shows it to be a perfect and beautiful spiral, "with a central stellar nucleus," and numerous "star-like condensations" in the convolutions of the spiral.

M. 75. 20^{h} $0^{m}{\cdot}2$, S. 22° 12′.—Messier considered it to be a mass of very small stars. Sir William Herschel resolved it into stars. Smyth calls it a globular cluster. Webb says, "Bright nucleus with low power."

M. 76. 1^{h} $36^{m}{\cdot}0$, N. 51° 4′.—A little north of the star 54 Andromedæ. A double nebula, the companion being No. 193 of Herschel's I. Class. Messier thought it was a cluster, but Sir William Herschel considered it irresolvable. Lord Rosse found it spiral with two bright centres. Webb says, "Curious miniature of M. 27 and like it, *gaseous*." A photograph by Dr. Roberts in Nov. 1891 "shows the two nebulæ to be one only . . . The figure of the nebula suggests that it is a broad ring seen edgewise."

M. 77. 2^{h} 37^{m}·6, S. 0° 26′.—About 1 degree south following δ Ceti. Sir John Herschel describes it as pretty large and irregularly round, brighter in the middle, with a nucleus, and partly resolved. Lord Rosse and Lassell thought it spiral. A photo by Dr. Roberts taken in Nov. 1892 shows "a stellar nucleus with projecting *ansæ* of dense nebulosity . . . and surrounding the *ansæ* is a zone of faint nebulosity surrounded by a broad nebulous ring which is studded with strong condensations resembling stars with irregular margins." The surrounding region is very devoid of stars.

M. 78. 5^{h} 41^{m}·6, N. 0° 1′.—About 2½ degrees following ζ Orionis. Webb says "singular 'wispy' nebula." Lord Rosse thought it possibly a spiral.

M. 79. 5^{h} 20^{m}·1, S. 24° 37′.—About 3½ degrees south of β Leporis. Seen by Sir William Herschel as a cluster about 3′ in diameter. Webb says: "Tolerably bright with my 64, blazing in centre; higher powers showed it mottled."

M. 80. 16^{h} 11^{m}·1, S. 22° 44′.—About midway between α and β Scorpii, and a little north of 19 Scorpii. Sir William Herschel thought it the richest and most condensed mass of stars in the heavens. Sir John Herschel, in his *Cape Observations* (p. 111), describes it as "a globular cluster; round; suddenly very much brighter in the middle to a blaze . . . stars = 14 m.; all resolved. Fine object." In May 1860 a temporary star of about 7th magnitude blazed out in the centre of this nebula, and by its light completely obscured the cluster. On June 16 of the same year it had completely disappeared and has not been seen since. A little north following the nebula are two known variable stars, R and S Scorpii.

M. 81. 9^{h} 47^{m}·3, N. 69° 32′.—In Ursa Major, about 10 degrees north preceding α. Described by Sir John Herschel as a remarkable, extremely bright nebula with a nucleus. Lord Rosse thought it very like the great nebula in Andromeda, and this is confirmed by a photograph taken by Dr. Roberts in March 1889 with an exposure of 3½ hours; but it is very much smaller in apparent size. This photograph shows the nebula "to be a spiral, with a nucleus which is not well defined at its boundary, and is surrounded by rings of nebulous matter." Huggins finds a continuous spectrum, like that of the great nebula in Andromeda.

M. 82. 9^{h} 47^{m}·6, N. 70° 10′.—About half a degree north of M. 81 (above). Sir John Herschel described it as a very bright ray and very large. Lord Rosse called it a most extraordinary object. Dr. Roberts's photograph shows that it "is probably seen edgewise with several nuclei of nebulous character involved." Huggins finds the spectrum continuous.

M. 83. 13^{h} 31^{m}·4, S. 29° 22′.—Sir John Herschel says "very bright; very large; suddenly much brighter in middle to a nucleus; large 3-branched spiral."

M. 84. 12^{h} 20^{m}·0, N. 13° 26′.—Closely preceding M. 86. Sir J. Herschel says, "very bright, pretty large; round; pretty suddenly much brighter in middle; resolvable."

M. 85. 12^{h} 20^{m}·4, N. 18° 45′.—A little following 11 Comæ Berenices.

M. 86. 12^{h} 21^{m}, N. 13° 30′.—In the nebulous region in Virgo. Sir John Herschel thought it probably resolvable.

M. 87. 12^{h} 25^{m}·8, N. 12° 56′.—In the nebulous region in Virgo. A little preceding M. 89. Sir John Herschel says, "very bright; very large; round; much brighter in the middle."

M. 88. 12^{h} 26^{m}·9, N. 14° 58′.—A long and faint nebula in the nebulous region in Virgo. Smyth called it "A long elliptical nebula." Lord Rosse thought it possibly spiral.

M. 89. 12^{h} 30^{m}·6, N. 13° 6′.—In the nebulous region in Virgo. A little following M. 87. Sir John Herschel says, "pretty bright; round; gradually much brighter in middle."

M. 90. 12^{h} 31^{m}·9, N. 13° 42′.—In the nebulous region in Virgo. A little north of M. 89. Sir John Herschel says, "pretty large; nucleus."

M. 91. In the nebulous region in Virgo. Sir John Herschel doubts the existence of this nebula.

M. 92. 17^{h} 14^{m}·1, N. 43° 15′.—In a rather blank space in Hercules, about 6 degrees north of π. Messier found it easily visible with a telescope of one foot in length. Sir William Herschel found it a brilliant cluster of 7′ or 8′ in diameter. Sir John Herschel described it as "a globular cluster, very bright and large and well resolved into small stars." Webb says, "a very fine cluster though not equal to M. 13; less resolvable; intensely bright in centre." A photograph by Dr. Roberts taken in May 1891 shows the cluster involved in dense nebulosity. But Prof. Barnard finds that there is no trace of any real nebulosity in any of the great globular clusters as seen with the great Yerkes telescope.

M. 93. 7^{h} 40^{m}·3, S. 23° 28′.—A little north following ξ Puppis (Argo). Sir William Herschel resolved it into stars 8–13 magnitude. Smyth calls it "A small galaxy cluster," and says "This neat group is of a star-fish shape, the *s. p.* portion being the brightest." Webb says, "Bright cluster in a rich neighbourhood."

M. 94. 12^{h} 46^{m}·2, N. 41° 40′.—A little to the north of a line joining α and β Canum Venaticorum. Sir John Herschel describes it as large, very bright, and irregularly round, with bright nucleus, and barely resolvable. Lord Rosse saw a dark and a bright ring round the nucleus and thought it probably spiral. A photograph was taken in May 1892 by Dr. Roberts which shows the stellar nucleus and rings, but he says, "I am unable to trace any spiral structure on the photograph."

M. 95. 10^{h} 38^{m}·7, N. 12° 13′.—About 4 degrees north following ρ Leonis. Discovered by Méchain in 1781. Sir John Herschel says, "bright; large; round; pretty gradually much brighter in middle to a nucleus." Lord Rosse saw two ellipses and the centre perhaps resolvable.

M. 96. $10^{h}\ 41^{m}\cdot 5$, N. 12° 21′.—About one degree following M. 95. Discovered by Méchain in 1781. Sir John Herschel says, "very bright; very large; a little elongated; very suddenly very much brighter in the middle, barely resolvable." Smyth says (speaking of this and M. 95), "another round but not equally well-defined nebula, large and of a pale-white colour." A little north of M. 96 are two faint nebulæ, Nos. 17 and 18 of Sir William Herschel's Class I.

M. 97. $11^{h}\ 9^{m}\cdot 0$, N. 55° 34′.—About 2 degrees south following β Ursæ Majoris. Described by Sir John Herschel as a remarkable planetary nebula, very bright, very large, and about 160 seconds of arc in diameter. Lord Rosse resembled it to the face of an owl, and it has since been known as the "the owl nebula." A photograph by Dr. Roberts, taken in April 1895, shows it of an elliptical shape, about 203 seconds in diameter, with a 15th-magnitude star in the centre, but no other stars in the nebula. He says, "The star seen by both Lord Rosse and Dr. Robinson has disappeared."

M. 98. $12^{h}\ 8^{m}\cdot 7$, N. 15° 27′.—Closely preceding the star 6 Comæ Berenices. Smyth says, "A fine and large but rather pale nebula . . . elongated in the direction of two stars."

M. 99. $12^{h}\ 13^{m}\cdot 7$, N. 14° 58′.—A little south following 6 Comæ Berenices. Sir John Herschel describes it as a very remarkable object, bright, large and round. Lord Rosse found it to be a wonderful spiral. A photograph by Roberts in May 1896 confirms the spiral character, and shows "many star-like condensations in the convolutions."

M. 100. $12^{h}\ 17^{m}\cdot 9$, N. 16° 23′.—About 2 degrees north following 6 Comæ Berenices. Discovered by Méchain in 1781. Smyth thought it "globular," but Lord Rosse found it to be a spiral, with the centre a planetary nebula. A photograph by Dr. Roberts taken in May 1896 shows it to be a "strikingly perfect" spiral, and with a sharply stellar nucleus "in the midst of faint nebulosity."

M. 101. $13^{h}\ 59^{m}\cdot 6$, N. 54° 50′.—About 5 degrees following ζ Ursæ Majoris. Sir John Herschel describes it as pretty bright, very large, and irregularly round. Smyth speaks of it as "one of those globular nebulæ that seem to be caused by a vast agglomeration of stars rather than by a mass of diffused luminous matter." But Lord Rosse found it to be a large spiral, 14′ in diameter, and his drawing agrees fairly well with a photograph taken by Dr. Roberts in May 1892, which shows "a well-defined stellar nucleus" with the usual convolutions and "star-like condensations."

M. 103. $1^{h}\ 26^{m}\cdot 6$, N. 60° 11′.—About 1 degree north following δ Cassiopeiæ. Sir John Herschel describes it as a bright cluster, pretty large, round and rich, with stars 10th and 11th magnitudes. A photograph by Dr. Roberts taken in December 1892 shows stars down to about 15th magnitude. It contains bright and faint stars, but the cluster does not seem to be a very rich one. Smyth called it a "fan-shaped group," and the photograph confirms this description. J. E. Gore.

Dublin, 1902, June 13.

The Lunar Atmosphere.

In a paper recently presented to the Académie des Sciences on the occasion of the appearance of the fifth and sixth parts of their 'Photographic Atlas of the Moon,' MM. Loewy and Puiseux call attention to several points arising from a general consideration of their photographs. Among the most interesting of these is one which has an important bearing upon the arguments in favour of the past existence of an atmosphere of considerably greater density than that which the Moon now possesses.

A comparative study of the surfaces of the Earth and Moon shows that whilst the former has been moulded largely under the influence of considerable lateral pressure, the crust of the Moon everywhere bears evidence of a superficial tension, as indicated by the cracks and dislocations found upon its surface. Now it has been recently shown, by Dr. Charles Davison, that in a globe cooling steadily by the dissipation, under uniform conditions, of its internal heat, there will be an outer shell subjected to pressure, whilst the material beneath this will be in a state of tension, increasing to a maximum, and then decreasing again as we proceed towards the centre. The thermal data obtained for the Earth fix the stratum of zero-tension at 8 km., and that of maximum tension at 110 km. If there is no change in the external conditions under which the cooling proceeds, the depth of these strata should vary as the square root of the time elapsed since the superficial solidification. But if, from the operation of external causes, a more rapid rate of cooling is set up, the layer in which the maximum loss of heat takes place rises towards the surface, the pressure is then diminished, and, if the process is carried far enough, the surface itself may be brought into a state of tension. When the external conditions once more become steady a region of pressure is again developed in the outer stratum; but this region is at first very thin, and the effects of the pressure may for a long time be completely masked by the tension of the strata beneath.

The state of the lunar surface would seem to indicate that some cause of this nature has been in operation subsequently to the solidification of the crust. It is almost certain that the Moon at one time had an atmosphere of quite appreciable density, and the disappearance of this atmosphere would seem to offer just such an event as is required to account for a considerable increase in the rate of loss of internal heat at some remote period of its history. If the cooling of the surface were already well advanced, it is clear that this increase would be more rapid in the equatorial than in the polar regions, which would have a much lower temperature to start with, and the greater superficial tension developed in the equatorial part of the surface would result in a diminution of curvature, which might be sufficient to cause an inflow of any liquid yet remaining at higher latitudes. It accords with this that we find the seas chiefly situated in the equatorial regions, whilst the traces of erosion still to be detected indicate a flow in both hemispheres directed towards the equator. It should be remembered that there are reasons for believing that the axis of rotation once occupied a position which would bring the principal seas more nearly into the equatorial zone than they are at present.

MM. Loewy and Puiseux also refer to the photographic evidence of the occurrence of periodic changes in the appearance of certain spots, depending upon the progress of the lunar day. They have previously called attention to the fact that in some spots there is a periodic change of contour of this description; they now specially consider an oval plateau of some 270 sq. km. near Vitruvius A, in which there is a periodic change of brightness, whilst the contour remains unaltered. This plateau at sunrise is darker than the surrounding plain, merges into it under meridional illumination, and finally reappears as an increasingly bright spot under the rays of the setting Sun.

The fact that such periodic changes do take place has long been known to selenographers. Mädler drew attention to their occurrence in various regions, one south of the Mare Crisium and another north of Hyginus, and tells us that in his day many attributed them to vegetation; an explanation which he, however, does not favour, for the reason that "a vegetation without air or water lies entirely beyond the range of our comprehension." He is constrained to leave them as an unsolved enigma.

The same theory has, however, been recently revived by Prof. W. H. Pickering, who, in Vol. 32 of the *Harvard Annals*, gives a list of 33 variable spots to which his attention was directed in 1893, whilst observing at Arequipa; and in the May and June numbers of the *Century Magazine* he has given a popular account of his more recent observations at Mandeville, Jamaica, where, he tells us, the atmospheric conditions are such that one may see at a glance, through a 5-inch telescope, details which are never seen, even under the most favourable circumstances and with the largest telescopes, at Cambridge. But to account for vegetation it would seem necessary that there should be some atmosphere, and attempts made at Harvard and elsewhere to obtain positive evidence of its existence by a series of rapid measures of a wide double-star as the components approach the Moon's limb before occultation, have led only to negative results. In this way Prof. Comstock fixes a superior limit for refraction upon the dark side of the Moon at $2H = 0''\cdot 2$, whilst Prof. Pickering, from observations on the bright limb, fixes the corresponding limit at $2H = 0''\cdot 4$. If there should be an atmosphere it will probably contain some vapours capable of condensation in the intense cold of a lunar night, and we should accordingly expect that it would be more easily detected on the bright limb than on the dark. But most selenographers have believed that they could detect traces of the existence of a very rare atmosphere, sometimes in the prolongation of the cusps by twilight, sometimes in effects visible at sunrise in the interior of a deep crater, and sometimes in local failure of definition under higher illumination. Prof. Pickering now adduces evidence obtained during an occultation of Jupiter, when a dark band was observed, both visually and photographically, crossing the planet nearly at right angles to the belts, tangential to the Moon's limb, and reaching to some 3″ above it. This he attributes to a light haze rising for a few miles above the lunar surface, where it was illuminated by the Sun.

He also, in the same connection, calls renewed attention to the crater Linné and the white spot surrounding it. The crater itself is now so small that it can usually be seen only under very low illumination; but it is surrounded by a white spot which forms one of the most conspicuous objects on the floor of the Mare Serenitatis. Prof. Pickering states that this spot does not become visible until a day, or a day and a half, after sunrise; but it is certainly sometimes to be seen before this, for the writer has seen and measured it within 17 hours of computed sunrise, though it was then but little brighter than the surrounding plain. Measures of the diameter of this spot, taken at various times and by different observers, have shown perplexing variations, ranging between $1''\cdot 81$ obtained by Schmidt in 1866, and $11''\cdot 61$ by Birt in the same year. From a discussion of his own observations Prof. Pickering makes out a strong case for a seasonal variation, the spot decreasing in size until about a day after the Sun has passed the meridian, and then increasing again. Dr. Klein thinks that the dependence of the size of the spot upon the position of the Sun is not sufficiently demonstrated; but an incomplete series of measures undertaken by the writer, in response to an appeal by Prof. Pickering, appear, at present, to give results in accordance with his view. When, however, Prof. Pickering compares these variations with those of the polar caps of Mars, and finds in this, and in other bright spots and regions, evidence of the existence of snow on the Moon, he would seem to be going a little further than is warranted by the facts hitherto adduced.

A further analogy with Mars is suggested by a system of dark "canals" in and around the walls of the crater Eratosthenes. These appear and disappear during the progress of the lunar day, and it is suggested that they are due to some low type of vegetation. Prof. Pickering will, perhaps, hardly expect that all his conclusions should be at once accepted by those who have not had his exceptional opportunities for observation; but there can be no doubt as to the value of the work he records, or of the new interest he has given to the study of the Moon. S. A. SAUNDER.

Fir Holt, Crowthorne,
1902, Aug. 16.

Variable Stars of Long Period.[*]

THE problem presents itself to every owner of a telescope, how to use it in order to obtain results of real scientific value. The number of telescopes of moderate size, 10 to 30 cm. aperture, is very large, and out of all proportion to the results obtained with them. Such investigations as the motion of stars in the line of sight, micrometric measures of position, and details in the surfaces of the planets are now pursued with such skill, aided by the most powerful telescopes in the world, that work with small instruments is, in most cases, wasted. One kind of work, the observation of variable stars by Argelander's method, is usually recommended for small telescopes, but here the results will be of but little value unless made according to some definite system. It has been the aim of the Harvard College Observatory to render such observations of real value, as will be seen by the references given below to its publications. Visual observations, by Argelander's method, of variable stars of small range are of little value, since both the accidental and the systematic errors may amount to several tenths of a magnitude, or a large portion of the total change. The photometer in use here with the 15-inch Equatorial gives results whose probable error is less than three one hundredths of a magnitude. A variation of a tenth of a magnitude can be determined with certainty. Observations on a single night, with such an instrument, are more valuable in determining small and rapid variations, than those extending over months, made by the eye alone[†]. Again, if the variation is regular, it is much better to observe the light-curve once for all with the photometer, when subsequent observations will only be required to determine the period and its variations, if any. On the other hand, variables of long period and large range can be observed to great advantage by Argelander's method. It is only necessary to select stars very slightly brighter and fainter than the variable, and estimate the intervals in grades, taking great care to identify correctly both the variable and the comparison stars. When the range is large, the mere statement, that the variable is slightly brighter than a given star and fainter than another, is nearly as valuable as an estimate in grades. The number of known variables of long period is now so great, that many observers could occupy themselves in making such observations. Each star should be observed by some one at least once a month, and preferably once a week, and observations when the star is visible only in the morning twilight are of special value. The greater portion of stars of this class appear to be irregular in their variations, so that observations extending over many years will be required to determine the nature of their changes.

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[*] Extract from the 'Archives Néerlandaises des Sciences Exactes et Naturelles.'

[†] See 'Harvard Circulars' Nos. 23, 30, and 41.

When the stars are brighter than the tenth magnitude, the charts of the Bonn Durchmusterung are indispensable in identifying the regions and the comparison-stars. The recent republication of these charts, which had become out of print, now brings them within the reach of all. The size of the charts and the minuteness of the images of the stars, however, render it difficult to use them at the telescope. Accordingly, photographic enlargements on a scale of 1′=0·1 cm. have been made of the region 3° square surrounding 69 of these variables. Copies of these enlargements will be given free of cost to experienced observers, who are ready to cooperate in these observations (Circular No. 53). For stars from the tenth to the thirteenth magnitude, Hagen's Atlas Stellarum Variabilium, Series I, II, aud III, is equally indispensable.

Such observations as have been described above, together with a vast number of similar observations which have been made during the last half-century, would have but little value unless reduced to a uniform system, so that they may be compared with one another. A large part of the photometric work of the Harvard College Observatory during the last twenty-five years may be used for this purpose. Rather more than a million settings have been made with the meridian photometer at the stations in Cambridge and Arequipa, Peru. The light of all the stars from the North to the South Pole, of the seventh magnitude and brighter, has been determined upon a uniform system of stellar magnitudes. Besides this, all stars of the ninth magnitude and brighter, in zones 20′ wide, and at intervals of 5° in declination, have been measured according to the same system. We have thus about forty thousand stars scattered in all parts of the sky, whose light is known. This work is now being extended to the fainter stars. With the polarizing photometer attached to the 15-inch Equatorial, two stars can be compared photometrically, even if they are as faint as the thirteenth magnitude, provided that they are within half a degree of each other. During the last three years, the writer has made nearly two hundred thousand photometric comparisons with a telescope of 30 cm. aperture, mounted horizontally. Stars as faint as the thirteenth magnitude can be compared with this instrument when within forty minutes of the meridian. The scale of stellar magnitudes in use for the brighter stars may thus be extended to the thirteenth magnitude. This scale will be still further extended by the cooperation which has been effected by the Yerkes, Lick, McCormick, and Harvard Observatories. In this work similar photometers are used on telescopes having apertures of 100, 90, 65, and 38 cm. It is expected that in this way the scale of magnitudes will be extended to the faintest stars visible in the largest telescopes as yet constructed, and that we shall have accurate standards of magnitudes distributed over the whole sky for stars as faint as the sixteenth, or even the seventeenth magnitude.

Having thus secured a uniform and consistent standard scale, the next problem is to reduce the observations of variables to it. The original observations of the great work of Argelander on variable stars, from the years 1838 to 1867, were published in 1869. As these observations were not reduced, but little use could be made of them. A reduction to the photometric scale has recently been published in Volume XXXIII. of the Harvard Annals. Similar reductions of the observations of Schmidt, and of the early observations of Schönfeld, appear in the same volume. Valentiner has recently published the much more extensive, later work of Schönfeld, and it is hoped that the results may be similarly reduced to the photometric scale. The observations made at Harvard, and some of those made elsewhere, of sixteen circumpolar variables, from 1889 to 1899, have been reduced to the photometric scale and published in full in the 'Annals,' Vol. XXXVII. Part I. Similar observations of 56 other variable stars of long period are now in the hands of the printer, and will form Part II. of the same volume.

A large piece of work is now in preparation here, which, if carried out, will greatly facilitate the reduction of observations of variable stars. In the Catalogues accompanying Hagen's Atlas all of the stars on each chart are arranged in a sequence, thus giving the relative, although not the absolute, brightness. It is proposed to measure photometrically a number of stars on each of these charts and thus reduce the magnitudes to the photometric scale. This has already been done for a large number of the charts. If it is completed, useful observations can be made of any of these variables, very simply. It will only be necessary to select any stars on the chart which are slightly brighter and slightly fainter than the variable. The record will consist of the date and the catalogue numbers of these stars. The observations will be improved if the intervals are estimated, and the hour and minute also noted. For long period variables of large range, however, this is not essential. If such observations, for which no apparatus but an ordinary telescope is required, are made systematically, results of great value will be obtained. If all variables of long period could be observed according to this system, our knowledge of the laws regulating these curious and interesting objects would be greatly increased. E. C. Pickering.

Harvard College Observatory,
Cambridge, Mass., U.S.A.
1901, July 27.

Measurement of Close Double Stars.

Dr. Hermann Struve, Director of the Königsberg Observatory, has recently advanced a doctrine, in reference to the measurement of close double stars, which will hardly be acceptable to double-star observers who have the use of telescopes of the largest sizes, and good atmospheric conditions. He states it as his opinion that, even under the most favourable circumstances, when the distance between the components of a double star is less than 0″·40, approximate estimations only of the position-angle can be obtained; and when the distance is under 0″·50, that it has not been demonstrated that the distance is given more accurately by micrometer-measurement than by mere estimation; and, further, he regards the results of the attempts which have been made to supply this demonstration as purely illusory.

Every one of experience will, I think, be ready to admit that when an observer has become accustomed to a given telescope and given eyepieces in connection with it, he may acquire an accuracy in the estimation of small angular distances which is surprisingly near the truth. But before he can make such estimates he must acquire a knowledge of the scale of his instrument and the effect of the eyepieces employed. Now this knowledge can only be obtained practically by measuring distances of the same order of magnitude as those to be estimated, or by relying upon such measures already made by an observer of experience. In either case the estimate, to be reliable, must be based upon measured or measurable quantities.

The angular diameter of the central disc of the image of a star of the fifth or sixth magnitude, as formed by the 36-inch refractor of the Lick Observatory, is not more than 0″·10; the theoretical value is 0″·13. The discs for faint stars are somewhat smaller than for bright stars. The apparent thickness of the spider-threads used as micrometer-wires in this instrument is very approximately 0″·10. When the atmospheric conditions are favourable, giving good images of the stars, and when a sufficiently high power is used, the micrometer-wire, even when fully illuminated, may be seen as a dark wire crossing, and almost concealing, the central disc of the star which it bisects. The appearance of the wire as a dark line crossing the disc makes the bisection of the disc perfectly definite; it also makes perfectly definite the simultaneous bisection of two separated discs, as in the measurement of position-angle by means of a single wire, or of the distance by the use of two.

A double star having components of equal or nearly equal brightness can be measured with this instrument under such atmospheric conditions as often prevail at Mt. Hamilton, whenever the distance between the components is greater than the thickness of the micrometer-wire. When the distance is somewhat less than this, an elongation of the image is still very apparent, and an approximate determination of the position-angle and distance may be obtained by measurement, and not by mere estimation. When the apparent distance becomes as small as 0″·05, the images coalesce and appear almost or quite round, even with the highest powers that are used, viz. 2400 and 2600. For distances greater than 0″·15 the images of the stars under good atmospheric conditions are clearly separated, and then measures can be made with certainty. For a distance of a third of a second the images are separated by a space several times their own diameters, and there is then no excuse for estimating such a distance rather than measuring it. W. J. Hussey.

[Professor Hussey here gives my own experiences very closely, merely changing his 0″·10 to 0″·13, and 0″·13 to 0″·17. The contention of Dr. Hermann Struve is contrary to all my experience, and if worked out for a 7-inch would mean that stars separated 2″·0 could not be micrometrically measured. When looking over Dr. Struve's recent measures I was struck with the absence of close pairs, and from his notes came to the conclusion that he was using wires 0″·25. This would account for much of his difficulty.—T. Lewis.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Word "Almanac."

Gentlemen,—

Mr. Johnson has furnished us, in your last number, with much interesting information respecting early almanacs, but I must beg to demur to his etymology of the word. It has been much disputed, and has been usually thought to be of Arabic origin, like many other words of which the first syllable is *al*, the Arabic definite article. At any rate, the fact that it is nearly the same in most European languages is proof sufficient that its root is not to be found in any of them. Prof. Skeat thinks it may be Arabic, but that they borrowed the second syllable from the Greek *μήν*, a month; and we know, by the analogy of *almagest*, that they may have formed a scientific word in that way. On the other hand, the earliest known use of any cognate form of the word is in Eusebius, who writes in his *Præparatio Evangelica* (iii. 92, D), speaking of the Egyptians and their astrological predictions: *ὧν καὶ ὀνόματα ἐν τοῖς Ἀλμενιχιακοῖς φέρεται*, ... This led Lenormant to the very probable conjecture that the word may in fact be of Coptic origin—*al* in that language signifying calculation, and *men* memory; so that the whole word would mean "remembered" or "recalled" calculation. Of this Littré remarks: "L'étymologie égyptienne a une certaine probabilité."

With regard to the calendars on square sticks, said to be used by our Saxon forefathers under the name of "almonaght"—that is, all-moon-heed, by which they took *heed* or regard of all the months in the year, as stated in the late E. Newenham Hoare's 'English Roots' (2nd edition 1856*, p. 20),—the only known authority, I believe, for this statement is Richard Verstegan, *alias* Rowlands, who published at Antwerp, in 1605, "A Restitution of Decayed Intelligence, in Antiquities, concerning the most noble and renowned English Nation," dedicated to the King, James I. At p. 58 of this quaint production we read of the Saxons in England: "They used to engrave upon certain squared sticks about a foot in length, or shorter or longer as they pleased, the courses of the moons of the whole year, whereby they could always certainly tell when the new moons, full moons, and changes should happen, as also their festival days; and such a carved stick they called an *Al-mon-aght*, that is to say, *All-moon-heed*, to wit, the regard or observation of all the moons, and here-hence is derived the name of *Almanac.*"

A considerable part of this book is occupied with etymologies, many of them exceedingly fanciful, as, for instance, when he attempts to prove the great antiquity of the English language from the supposed identity of "babble" with "Bable," and invents a story for the origin of the proper name William, that it was first given to a soldier who killed a Roman in battle and put on his (golden) helm or helmet. It is impossible, for the reasons given above, to accept his derivation of almanac, which appears to be a mere conjecture of a very fanciful kind.

Roger Bacon was, I believe, the first English writer to use the word almanac, which he spells, as it is in French and German, "almanach." The spelling "almanack" or "almanacke" seems to have begun in the sixteenth century; but "almanac" is now almost universal, excluding the unnecessary final *k*.

Yours faithfully,

Blackheath, 1902, Aug. 8. W. T. Lynn.

The Red Spot Region of Jupiter.

Gentlemen,—

In reference to Mr. Phillips's letter in the last number of the *Observatory*, I beg to state that the spot he and Mr. Denning saw on June 28, under 18°–20°, had *nothing to do* with the interesting marking of the opposition of 1901, which extended over 30 degrees, and was called by me "*pyramid-spot*," because it consisted of pyramids, first in the number of one, then of two, and at last of three. The *real* pyramid-spot is still near the Great Red Spot, where I measured its following end on August 13 to

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* The author was Dean of Waterford, and the above little work consists of two lectures delivered in 1855 to the Waterford Mechanics' Scientific Institute.

75°, which agrees well with Mr. Phillips's statement, that he found it under 81° 22 days before. For the movement of the pyramid-spot was of 48° within *85 days* last year, so that it is quite impossible for it to make the *same* distance in *8 days* now, as it must be if it were identical with the spot under 19° on June 28. Yesterday I found that the *f* pyramid stood just above the right shoulder, the central pyramid formed its prolongation into the S.T.B., and the *p* pyramid is probably comprised in the dark matter which is forming the southern border of the Great Red Spot in the S.T.B. Particulars of my interesting observation will be found in the next number of our *Astronomische Rundschau*.

Manora-Sternwarte,
1902, Aug. 14.

Yours faithfully,
LEO BRENNER.

Lexell.

GENTLEMEN,—

As there seems to be a possibility (one cannot call it more) that Lexell's "lost comet" may be seen again shortly, a few words respecting that mathematician and astronomer may be interesting at the present time. Mädler, in his 'Geschichte der Himmelkunde,' vol. i. p. 473, remarks :—"Der Komet von 1769 *–1770 verdient den Namen das Lexell'schen mit demselben Rechte, wie wir den von kürzester Umlaufszeit den Encke'schen nennen."

Anders Johann Lexell was born on the 24th of December, 1740, at Abo, in Finland, which was then a province of Sweden. He was educated at the University there, and became a Professor, shortly afterwards removing to St. Petersburg. Under the direction and encouragement of Euler, he calculated the orbit of the comet of 1769 (discovered a few days before the usually-assigned date of the birth of Napoleon I., though this may have really been a year earlier), and published the result under the title "Recherches et Calculs sur la vraie orbit elliptique de la Comète de 1769": the elements calculated by him do not differ greatly from those afterwards determined by Bessel, and give a period of about 2000 years to the comet. He also wrote a considerable number of papers on subjects in pure mathematics, which are all that Larousse mentions; also (in conjunction with young Euler and Krafft), a tract 'Theoria motuum lunæ, una cum tabulis astronomicis'; reduced several observations for longitude determinations; and calculated the parallax of the Sun from the transits of Venus, his final result being 8″·63. His work on the comet of 1770 is entitled 'Reflexions sur le Temps périodique des Comètes en général, et principalement de la Comète de 1770.' I need not go into the history of this comet here, which is well known. M. Schulhof thinks it likely that one discovered by

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* This must be a slip on the author's part; the comet was discovered by Messier on the 14th of June, 1770, and passed its perihelion on the following Aug. 13. It was the only comet seen in 1770, as 1770 II. was not discovered till 1771, Jan. 10, though it passed its perihelion in 1770, Nov. 22.

Prof. Swift on the 20th of August, 1895, and calculated to be moving in an elliptic orbit with a period of little more than seven years (so that it should be due again about the present time, but, unfortunately, in an unfavourable position for observation), may be identical with it, after having undergone many vagaries in its course in consequence of planetary perturbation.

Mädler remarks that this work of Lexell was "die wichtigste seiner Schriften"; but probably that on the planet afterwards called Uranus is at least as important. It is well known that when Herschel discovered that planet, he regarded it as a comet, and it is the last body of the kind referred to in Pingré's monumental 'Cométographie.' The first to prove its planetary character was Lexell. His 'Recherches sur la nouvelle Planète découverte par Herschel' appeared in 1783; and he died on the 24th of December in the following year, 1784. Lexell was in England when Uranus was discovered; and, as an alternative to Herschel's name "Georgium Sidus," proposed "the Neptune of George III.," remarking that the word "Sidus" was not properly applicable to a planet.

Yours faithfully,

Blackheath, 1902, Aug. 13. W. T. LYNN.

Edward Pigott and the Comet of 1807.

GENTLEMEN,—

The only comet seen in the year 1807 was a large one, visible to the naked eye, and appeared in the month of September. Those were not days of rapid scientific communication ("Schade," says Bessel on the subject), and it was discovered independently by several persons at different times, the first probably being Pons, on the 20th of September. The claim made by an Augustinian monk at Castro Giovanni, in Sicily, to have seen it more than a week earlier was not made until more than two months afterwards *, and cannot, it seems to me, be accepted as genuine.

This comet has a melancholy interest for us as Englishmen, in that it was the subject of the last astronomical observation of Edward Pigott, who discovered the first comet of 1783, and the variability of η Aquilæ and other stars, a class of observation in which he was closely associated with his younger friend, John Goodricke, the discoverer of the periodic variability of Algol. Pigott, in the latter part of his life, resided at Bath, and whilst there independently detected the comet of 1807 on September 28. This he communicated to Sir W. Herschel †, after which nothing whatever appears to be known about him. His brother, Charles Gregory Pigott, took the name of Fairfax on coming into possession of the Gilling Castle property in 1793, and married, in the following year, Mary, sister of Sir Henry Goodricke and of John

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* Zach's *Monatliche Correspondenz*, 1809.

† *Phil. Trans.* for 1808, p. 145.

Goodricke the astronomer, who had died in 1786, as is recorded by his friend Edward Pigott in the *Phil. Trans.* for that year (vol. lxxvi. part 1, p. 424) in these terms:—"This worthy young man exists no more; he is not only regretted by many friends, but will prove a loss to astronomy, as the discoveries he so rapidly made sufficiently evince; also his quickness in the study of mathematics was well known to several persons eminent in that line." Some of his work must have been prosecuted under difficulties, as he was deaf and dumb from birth. This is mentioned in R. Wolf's 'Geschichte der Astronomie,' and also in his 'Handbuch der Astronomie.' I have not been able to find any contemporary English authority for this; but I am informed by Mr. C. A. Goodricke, of Croydon, author of 'History of the Goodricke Family' (to which I referred in your number for July), that it is true.

Yours faithfully,

Blackheath, 1902, Aug. 25.

W. T. LYNN.

OBSERVATORIES.

THE following notes on work at various Continental Observatories are extracted from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' and refer to the work during the year 1901.

BAMBURG. *Ernst Hartwig.*—The heliograph mounted in tower of the east wing was used regularly from June and a number of photographs were secured. The 6-inch refractor was used on 150 nights for observations of variable stars, including a fine series of Nova Persei. The weather was unfavourable for heliometer measures for parallax. Meteorology. The visitors numbered 400.

BERLIN. *W. Foerster.*—Copy for press is finished for "Results of Observations of 560 Stars, with proper motions for 233 Stars," also for "Estimates of Lunar Longitudes, Lunar Diameters, and of Solar Parallax from Occultations from 1894 to 1897." Portions of these have already appeared in the *Astronomische Nachrichten*, nos. 3754 and 3755. Time-service.

BERLIN. *J. Bauschinger.*—(Astronomical Computation Institute.) The work for 1901 comprises the *Astronomische Jahrbuch*, 1904; Elements and Ephemerides for small planets; collection of tables for theoretical astronomy; historical work, &c.

BONN. *F. Küstner.*—Revision of stars for the 1900·0 catalogue. The new refractor has been used for double-star work. Amongst the photographic work are negatives of η Coronæ (5·2 and 5·7 mag., dist. 0″·7) and OΣ 395 (5·8 and 6·7 mag., dist. 0″·7). Dr. Deichmuller has made progress with the new Ten-year Catalogue (1890). Time-service.

Breslau. *J. Franz.*—Work on Lunar Craters, diameters of Sun and Venus. Observations of Nova Persei.

Düsseldorf. *Wilhelm Luther.*—102 observations of 47 small planets, bringing the total observations made here by Brunnow, R. Luther, and W. Luther up to 2452, of which R. Luther made 1267. He also discovered 47 small planets.

Geneva. *R. Gautier.*—Time-service and "service chronométrique." The number of pocket-chronometers and watches, tested in 1901 was 306, a diminution of 222 on 1900. The equatorial was employed for observations of small planets and for Nova Persei. Meteorology.

Hamburg. *R. Schorr.*—The principal work appears to have been the preparation for publishing the Zone Catalogue. When this is completed it is hoped to start on a new reduction of Rumker's Catalogue (1836–55). Time-service: the mean error of the Time-Ball drop was $0^{s}\cdot23$ for the year. Meteorology.

Heidelberg. *W. Valentiner.*—The work with the meridian circle comprised

1086	observations of	Refraction Stars.
123	,,	Polars (α and δ Ursæ Min.).
10	,,	Polarissima.
25	,,	Sun.
532	,,	Comparison Stars.

The refractor was employed in observing the Gill zodiacal catalogue stars. The small circle was used for parallaxes. Variation of latitude work by the Horrebow-Talcott method, resulting in

1900	July	23	49° 23′ 52″·94	from	104	pairs.
	Aug.	20	52 ·88	,,	96	,,
	Sept.	16	52 ·86	,,	136	,,
	Oct.	13	52 ·89	,,	131	,,
	Nov.	20	52 ·91	,,	72	,,
	Dec.	14	52 ·96	,,	72	,,
1901	Jan.	12	52 ·93	,,	147	,,
	Feb.	18	53 ·05	,,	143	,,
	March	17	53 ·11	,,	39	,,
	April	21	53 ·14	,,	114	,,
	May	14	53 ·09	,,	211	,,
	June	14	53 ·05	,,	116	,,
	July	17	53 ·03	,,	106	,,
	Aug.	19	53 ·01	,,	95	,,
	Sept.	27	53 ·00	,,	66	,,
	Oct.	16	52 ·96	,,	135	,,
	Nov.	13	52 ·87	,,	174	,,

PUBLICATIONS.

A MANUAL OF ASTRONOMY*.—Of the books on the shelves of an astronomical reference library, Prof. Young's 'General Astronomy,' of which a second edition was published some years ago, is one of the most useful. The same author has also produced a smaller book, 'The Elements of Astronomy,' not so well known on this side of the Atlantic as the larger volume; and now, finding that there is need for a work of size intermediate to these, he has prepared this manual, which in size and appearance is not unlike the 'General Astronomy.' It contains over six hundred large octavo pages, it is illustrated with well-chosen diagrams and excellent reproductions of the latest and best astronomical photographs and drawings, and the printer and binder have exercised their skill and care so as to produce a book handsome and convenient to use.

The book treats of all the branches of astronomy. It describes instruments and celestial objects, methods and phenomena, in an accurate and complete manner, but concisely. Each subject is dealt with in a numbered paragraph—the last number is 705—with the heading in bold type, so as to catch the eye, and often with an incorporated paragraph in smaller type, giving the latest development of the matter under consideration or some special point in connection with it, which is brought into prominence by the change in the appearance of the page. This division into paragraphs does not, as might be thought, make the reading disjointed, as that of a dictionary or encyclopædia, for each section hangs generally on the preceding, so that the subject is carried on without discontinuity. Also in the margin there is found a *précis* of each paragraph, which is a great aid to the work considered as a reference book. Simple formulæ are given when required, but the intention of the author was to make the book available for persons not acquainted with the higher mathematics.

In our last number this book was mentioned by a contributor, who showed that it was brought well up to date by referring to the sections on Nova Persei and Eros. This recognition might be extended by calling attention to many other paragraphs. There is, for instance, a picture of the new transit-circle of the Washington Naval Observatory, which we see for the first time, and are glad to do so, as it involves some novel points of construction, and there is also an illustration of the altazimuth recently made by Messrs. Warner & Swasey for the same institution. The frontispiece is a plate of the Potsdam new equatorial of 31½ inches. Many quite recent suggestions and discoveries are mentioned, and in these cases it is easy to see the cautiousness of the author; he has evidently endeavoured to give all the information possible, but has refrained from making definite statements about matters which are as yet problematical. In the sections about Mars, for instance, he gives the planetary surface as drawn by Keeler, by Green, by Schiaparelli, and by Lowell, and a sketch of Mr. Lowell's views, but he is careful to state that certain speculations as to the so-called "canals" appear premature. As to the supposed ninth satellite of Saturn, he says, "The satellite, if it exists, is too small to be *seen* with any existing telescope, but we can photograph what is not *visible*," which seems a very fair way of stating the case. Andre's recent suggestion that all the stars are revolving as one system about the centre of gravity of the whole is mentioned, with the warning comment that the evidence for any such general revolution is far from conclusive. In writing of sun-spots, Prof. Young notes a suggested period of thirty-five years, but adds, "the time covered by the material is hardly sufficient to warrant a sure conclusion;" and to this it may be added that the whole of the two chapters about the Sun are written in a very clear and impartial manner, and give in a small compass an accurate statement of the many problems presented by our primary.

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* 'A Manual of Astronomy.' A Text-book by Charles A. Young, Ph.D., LL.D., Professor of Astronomy in Princeton University. 1902. Ginn & Co., U.S.A. and London. Price 10*s*. 6*d*.

It will be seen from the above that the book includes accounts of most of the recent advances in astronomy, and it is difficult to find things of this kind which are omitted. First, in the chapter on astronomical instruments, there is no mention of the Repsold registering micrometer, although two other devices to the same end, but which have not been brought into such practical effect as the Repsold, are given. Then, though the various ordinary systems of co-ordinates used to define astronomical position are described, there is no mention of the system of co-ordinates adopted for the astrographic catalogue in the preliminary chapter, nor in the section which describes the astrographic work. It is almost, if not quite, time that this system had found its way into the text-books. In the sections on telescopes, which, as a whole, are most instructive, giving, as they do, in simple language, the optical theory of the telescope, there is a small mistake of detail, which it may be well to correct. The 3-foot reflector at the Lick Observatory was not made by Dr. Common; we believe the original maker was Calver, and that it has since undergone modification at many hands.

We have, as yet, said nothing of a very important feature of of the book, considered as a text-book. At the end of each chapter there is a list of exercises, generally numerical examples, all very ingenious and thought-inspiring, in proof whereof we should like to quote many of them did space allow. The following are chosen almost at random :—

(1) How much longer would the Earth's period be if it were a mere particle?

(2) What is the magnitude of a star whose brightness is $\frac{1}{100,000}$ that of a first-magnitude star?

(3) If a luminous body were moving towards us with the speed of light, what would be the effect upon the apparent length of the portion of the spectrum between two given lines?

When it is remembered that the answers to these questions and many others like them can be readily and easily found from the book, and that it contains tables giving the latest determined information together with lists of stellar parallaxes, radial velocities, variable stars, and elements of the orbits of binary stars, it will be realized that this is certainly a work for the beginner in astronomy as well as the more advanced student to have ready to hand. H. P. H.

LOCKYER'S SHORT PERIOD IN SOLAR CHANGES AND RELATED PHENOMENA.—The connection between Indian rainfall and solar activity is still occupying the attention of Sir Norman Lockyer and Dr. W. J. Lockyer. In a memoir recently presented to the Royal Society attention has been specially devoted to an examination of the variations of atmospheric pressure over the Indian area, and it is considered by the authors, on a somewhat slender foundation (as it appears to us), that the variations show a distinct period of 3½ years. The records of solar phenomena were therefore examined to find, if possible, any element which had a period corresponding to this, and in the reprint, or possibly the abstract, of the memoir which appeared in *Nature* of July 10, curves showing the mean latitude of the sun-spots and the latitude of the total spotted area are given, together with those showing the variations in the total area of the sun-spots year by year, and of the number of prominences. The authors claim that these also show a 3½-year period; but it is hard to detect any such period in the curve of mean latitude, while the latitude of total spotted area is so easily influenced, one large group of spots being almost sufficient to change its sign, that its record as a physical element is of little value. The curve of total spotted area certainly has variations besides that of the eleven-year period, but any law governing these is as yet far from being established. The authors of the memoir guard themselves by saying that the short-period variation is not alone in its action, but that it is apparently influenced by the eleven-year and the thirty-five year periods, and add that further investigation is in progress.

The Moon and Rainfall.—It is not without intention that this note follows the preceding, for it illustrates the danger of basing a conclusion on limited data. Mr. MacDowall, who frequently writes on such subjects, made a communication to *Symons's Meteorological Magazine*, in which he deduced from the published figures that, in the years 1889 to 1900, the rainfall at Greenwich was, in each year, "greater about New Moon than about the middle of the time between Full Moon and Last Quarter." It must be added that Mr. MacDowall did not assert that this fact held universally, and for all years, but it was a conclusion that might easily have been drawn. Mr. William Ellis, with characteristic cautiousness, verified Mr. MacDowall's result, and carried the investigation back to the year 1862. He found that, in the first two decades following 1861, the difference of the rainfall at the two epochs had, in the mean, the opposite sign to that found in the original investigation, and that, in the decade 1892 to 1901, though the difference was in the same sense as in the following ten years, its value was considerably less, showing that the recorded figures did not warrant any physical conclusion being drawn from them. As Mr. Ellis says: "Meteorological investigations of this kind cannot be, as it were, rushed or settled by a few figures hastily thrown together. Nature does not so readily yield up many of her secrets."

Clouds and Weather Signs [*].—This little book of 31 pages (reproduced from *Knowledge*) is designed by the author to draw attention to what he describes as a much-neglected study, that of cloud-forms, which, as he says, from their unquestionable beauty and variety, have an interest of their own, apart from the fact of their observation being of such value in the forecasting of weather. The classification of cloud-forms into seven distinct modifications by Luke Howard, just 100 years ago, has been of the greatest value to meteorologists by enabling them readily to record the prevailing atmospheric conditions on a system which, easily understood, became, from its general adaptability and excellence, widely employed. Some of these divisions themselves include, however, varieties of form, to take more complete account of which later meteorologists have, at different times, proposed more extended systems of nomenclature; but, although these may have their value for the professional meteorologist, they are scarcely so suitable for the many amateur meteorological observers who do so large an amount of good and useful work. The more simple Howard system, so excellent as far as it goes, seems in general to be better adapted to ordinary practical work.

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[*] 'Clouds and Weather Signs (with Original Photographs).' By Commander D. Wilson Barker, R.N.R., F.R.Met.Soc. Office of *Knowledge*, 1901. 1s.

Commʳ Wilson Barker gives various interesting information on cloud-forms, the result of his own personal observation, and being, moreover, a very successful photographer, he has accompanied these remarks by copies of 16 original photographs of clouds which, whilst being excellent specimens of the different cloud-forms, are also beautiful pictures, having much artistic merit. Commʳ Barker remarks that "if an observer be a photographer as well, he will find open to him in Cloudland a fascinating field for study and a limitless variety of subject for his art," as, indeed, his pictures themselves show. To all who may be attracted to the study of atmospheric changes, this little book should prove very acceptable.

W. E.

THE "FLASH"-SPECTRUM.—A further contribution is made to the problem of the reversing layer in a paper by Dr. Humphreys, in the June number of the *Astrophysical Journal*, in which he gives the results he obtained in Sumatra in the eclipse of 1901. He was situated near the northern limit of totality, and worked with a concave grating of 30 feet focal length, without slit or lens. He gives a table of between 300 and 400 wave-lengths, reaching from λ 3118 to λ 5204. These are the strongest lines only of the "flash"-spectrum, and nearly all are identified with lines in Rowland's solar spectrum.

In view of the controversies on this subject which have arisen in the interpretation of various eclipse spectra since 1898, between Prof. Frost and Sir Norman Lockyer, and later between Mr. Evershed, Prof. Fowler, and Mr. Mitchell, we quote the conclusions of Dr. Humphreys:—"I have tried to find what relations, if any, exist between the lines found on my photographs and the spectra of the elements as produced under various other conditions; but, so far, no very close relation has been discovered, except the natural one that in general the heavier, and only the heavier, Fraunhofer lines appear as bright lines in the flash, and that the relative intensities in the two cases are roughly comparable. A line that comes out strong, both in the arc and spark spectrum, is likely to appear also in the flash-spectrum. Probably, too, the chances are more than even in favour of the appearance of the 'enhanced' lines; but the exceptions are too many and too decided to justify speculations based on this important phenomenon. . . . . . The upper and lower levels of the Sun's atmosphere contain, in the main, the same elements, and the Fraunhofer lines are true reversals of gaseous bright lines."

NOTES.

THE Lick Observatory Bulletin No. 20 contains notices of two gifts to the Observatory amounting to 3500 dollars.

The great demand for photographs has resulted in special means being taken to provide them, and the Observatory is now prepared to supply lantern-slides, transparencies, prints, &c. of any negatives in its collection, at moderate cost. It is assumed that such copies would be used for purposes of study and investigation; but the right to publish any or all copies without special permission from the Director of the Lick Observatory would not attach to the sale.

Then follows the announcement of variable velocities in the line of sight of ϕ Persei, η Geminorum, ζ Herculis, α Equulei, and o Andromedæ.

Bulletin No. 21 contains another hundred new double stars discovered by Prof. Hussey. Sixty of these are under $1''\cdot0$ separation.

IN continuation of his researches on planets and their satellites, Dr. T. J. See publishes, in No. 3806 of the *Astronomische Nachrichten*, the measures made by him of the satellites of Uranus and Saturn. These measures were made with the Washington 26-inch refractor. With respect to the measures of Iapetus, it is remarked that "The construction of the micrometer is such that in spite of the great distance of Iapetus it could be referred directly to Titan by sliding the eyepiece. By this process it is expected that a somewhat greater degree of accuracy is attained than by taking differences of declination by the micrometer, and differences of right ascension on a chronograph as is sometimes done."

In the *Astronomishe Nachrichten* No. 3676 Dr. See stated that the computed places of the satellites of Uranus, in the American Ephemeris, were 37° in advance of the observed. This Dr. See now states is an error due to the ambiguous wording of the Ephemeris, which still represents the motion of these satellites very well, and no great change in the places would be effected by a revision of the elements.

WE notice in the *Comptes Rendus*, July 28, that M. Deslandres is continuing his measurements of the rotation of the planets by means of the spectroscopic method. He uses a wide slit and moderate dispersion. M. Deslandres seems satisfied with the results, which will appear shortly; in the present paper he states that the rotation of Uranus is in a retrograde direction.

THE preliminary report of the Dutch Eclipse Expedition of 1901 to Sumatra states that the coronagraph negatives were generally spoiled by cloud, and only the short exposure on slow plates are of any use. Some fine prominences and details of the inner corona were obtained on a "Lumière jaune" plate in the 40-ft. coronagraph lent by the U.S. Naval Observatory. One good spectrograph was obtained. Professor Julius makes a special point of the doubling of all the chromospheric crescents, which he assigns to the phenomena of anomalous dispersion of the chromospheric light and not to instrumental causes. In the observations of the polarization of the coronal light it is shown that the light at a certain distance from the Sun's limb is more strongly polarized than that nearer the limb, and that outside this distance the polarization decreases again with the distance.

THE QUEENSLAND WEATHER BUREAU.—The office of Mr. Clement L. Wragge, who has done much useful work in collecting and tabulating meteorological statistics in Queensland, was abolished on June 30, owing to the necessity of economizing in the public expenditure.

WE have received from Prof. Schuster a copy of a memoir presented by him to the Royal Philosophical Society of Glasgow, entitled "The Evolution of Solar Stars," which consists of a summary of our spectroscopic knowledge of the different types of stars, and some valuable suggestions based thereon as to the construction of the Universe. We hope to give this adequate treatment in a subsequent number.

FROM AN OXFORD NOTE-BOOK.

THE important question raised by Sir David Gill, "Are the bright stars as a whole rotating with respect to fainter stars as a whole?" has received some attention at Oxford during the last month. Whether we should have started during the holiday season if it had not been for Mr. Carpenter, I don't know; but when he takes a holiday from his work at Durham he likes to come to the Observatory where he began life, and ask for something to do in memory of old times. Two years ago his "holiday task" resulted in showing how accurate are the paper charts published by the French observatories (see *Mon. Not.* lx. p. 618). This year it was suggested that he might commence an examination of our measures of stellar photographs to see whether any rotation of bright stars relatively to faint could be detected in the interval available, which amounts to 7 or 8 years. The work to be done before arriving at an answer is considerable, and Mr. Carpenter could not hope to complete it. But before he returned to Durham he had got so far as to find indications that a positive answer would come out; and with this fillip to our curiosity, the whole staff was put on the work for a few days to get out the result.

The result is most interesting. In the belt of stars about Declination +26° there *is* apparently a rotation of brighter stars relatively to faint; its amount is about 0^{s}·002 per magnitude per year, which is not unlike the quantity found by Sir David Gill; *but its sign is the opposite to that found by him.* There may of course be some mistake: a mistake of sign is easy to make, and when once made rather difficult to see, even when straight before one's eyes. But care has naturally been taken to get the right sign. As an independent check the P.M.s of the Groombridge stars given in the Introduction to the last Greenwich Ten-Year Catalogue (1890·0) were examined and seem to show an effect of about half the above amount and of the same sign. Is, then, the sign different in the N. and S. hemispheres? If so, the phenomenon is scarcely likely to be a rotation. The whole question is so important that it seems well to mention these results at once, with a word of caution as to the possibility of error in sign. The material on which they are based will of course be published as soon as possible.

Were it not that we have learned from experience to state the results of observation, irrespective of any difficulty in explaining them, I should hesitate to write the last paragraph; for it is not at all easy to form a conception of a state of things to suit these observed facts (assuming no mistake of sign). The coexistence of two actual rotations in opposite directions in the two hemispheres is not at all likely; and Sir David Gill has already remarked upon the insufficiency of "the usual hypotheses, such as parallactic motion due to translation of the Sun in space or systematic error in R.A. depending upon magnitude," to explain the facts concerning the southern hemisphere alone, without the extra complication of those introduced by the northern. A rather artificial explanation could be framed on the hypothesis that the brighter stars, distributed uniformly round the Sun, are moving with it in its parallactic motion, while the fainter, distributed near the plane of the Milky Way, are more stationary: but it is not satisfactory enough to give in detail, especially since "a diagraphm is necessesairy," as an examinee wrote recently.

In the recently published volumes of the Supplement to the *Encyclopædia Britannica* there is an important article on Astronomy by Simon Newcomb, well worth reading carefully. It deals more particularly with the best values at present available for astronomical constants; and it is with some surprise that we find 20″·53 given as probably the best modern value of the constant of aberration; for it is scarcely half-a-dozen years since a change from 20″·45 to 20″·47 was made as the result of a solemn Conference at Paris, and all our national ephemerides were upset in consequence. The probably best value of the Solar Parallax is given as 8″·762, which again differs sensibly from the 8″·80 adopted at Paris in 1896. However, the Paris Conference may serve a useful purpose in warning us that such devices have their dangers, and should be used with discretion.

HAS anyone who has mounted a good clock in the neighbourhood of a quantity of books found reason to suspect the latter of injurious influence in any way, *e. g.*, possible rusting of the clock mechanism? I heard of an instance the other day in which this effect was suspected; but a single instance is almost valueless, as the number of possible causes for rust is so large. A comparison of experiences, with either positive or negative results, might be of great value.

IN holiday times one may make the acquaintance of curious and miscellaneous literature. I have just been glancing through a little book called "Comets and Nebulæ," published by T. Nelson & Sons; no author or date given, but the latter may be inferred from the table of minor planets, which goes only to Thisbe (88). At this date our knowledge was, as the author is careful to point out, not quite complete, at least with regard to meteors. He announces that he will speak

> of the Milky Way and its myriad clusters of shining spheres; of the Comets, which flash through the infinite as on wings of flame; of the Nebulæ, which so long baffled the research of the astronomer; and of the Meteors, *which are still, in some respects, a difficult and curious problem* (p. 15).

The italics are mine; but I hope I do the author no injustice in supposing that he regarded most of the problems of astronomy as solved—witness his reference to the nebulæ, which had at that time been accepted as clusters, the spectroscope being still in the future. It may seem astonishing to us that people were so ready, 40 years ago, to believe that they had come to the end of astronomical knowledge; but such a state of things is not uncommon in the history of the human race.

The facts given in the little book are generally correct, for the date. The main purpose is to point out the moral benefits to be derived from a study of the heavens:—

> Instead of devoting ourselves to everyday follies and fashions, the little pursuits of little men, the baubles of ambition, and the gewgaws of wealth; instead of suffering ourselves to be led astray by the lust of the eyes, and the pride of life, we should turn our attention to a devout study of the wonders of creation.

As I write these words I can see from the window the monotonous succession of players striking from the first tee at St. Andrews, N.B., and I repeat to myself the words about "the little pursuits of little men, the baubles of ambition," and so forth. They afford substantial comfort, as I could not get a match this morning, and so have "turned my attention to a devout study of the wonders of creation," as represented in this little book.

A CORRESPONDENT sends me the following quotation from 'The Luck of the Vails,' by E. F. Benson:—

> "But to-day,—to-day!" she said, "at this hour a year ago evening fell, as it is falling now . . . . . and as a year ago a young moon, silver and slim, was climbing the sky."

Now, as there are 12½ lunations in a year, my correspondent writes, this is a little difficult to accept. If Mr. Benson had only made it two years ago!

THERE was in *The Sketch* for April 4, 1900, a story of buried treasure with an astronomical basis; and it is refreshing to find the facts for once represented with tolerable correctness. An old parchment indicates the spot where the treasure may be found: "the horned beast his eye and the bear his tayle do give the lyne: the distance be ninety and seven fathoms; the yeare be the yeare of fyre; the night be the eve of Martinmas; and the houre be twelve." The "horned beast" is a material unicorn in the grounds, but the bear is Ursa Major, and search is made at a spot 97 fathoms from the unicorn's eye towards the tail of the Great Bear at midnight on Martinmas eve, but at first nothing is found. Then the ingenious man of the story takes note of the fact that the "yeare of fyre," *i. e.* of the Great Fire of London, was some 230 years ago, and precession will have affected the star's place. He gets an astronomical friend to make the calculation for him, corrects the line accordingly, and triumphantly finds the treasure. If anyone likes to calculate the allowance to be made in feet and send the answer, I shall be glad to give it publicity. In the story we are only told that "my sum was not three feet out."

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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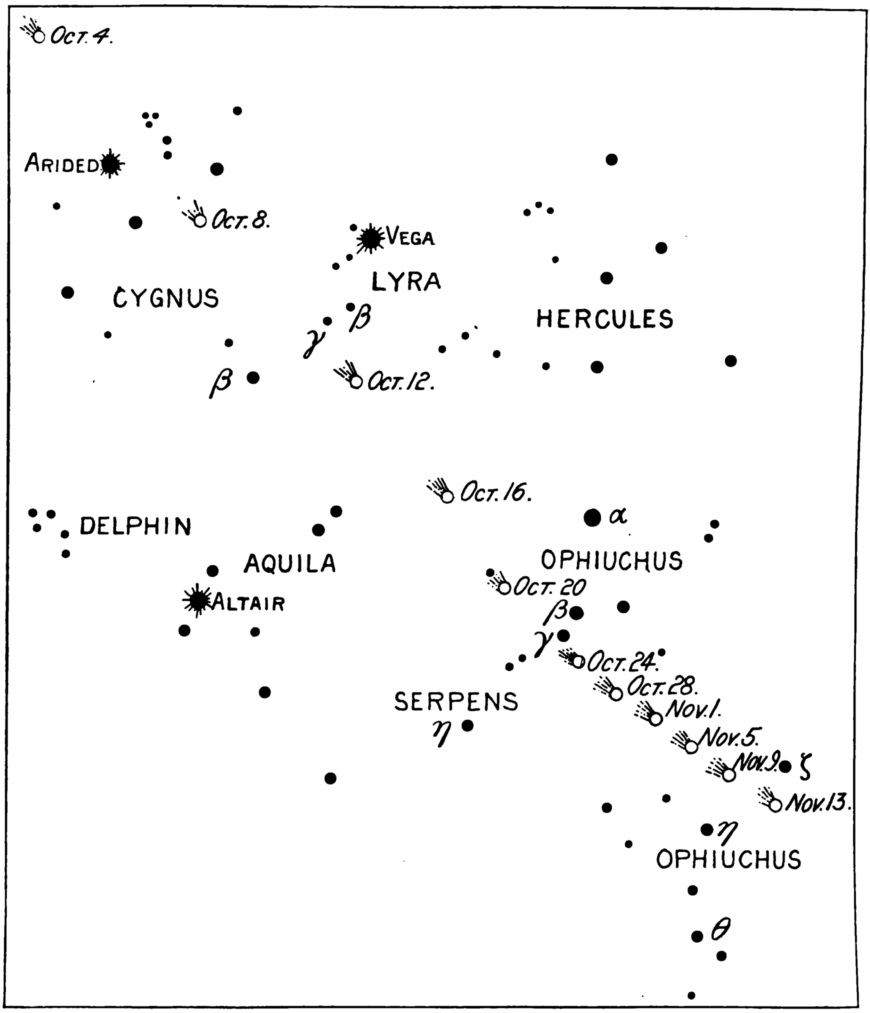

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Sketch-Map of the Comet's Path among the Stars from Oct. 4 to Nov. 13.

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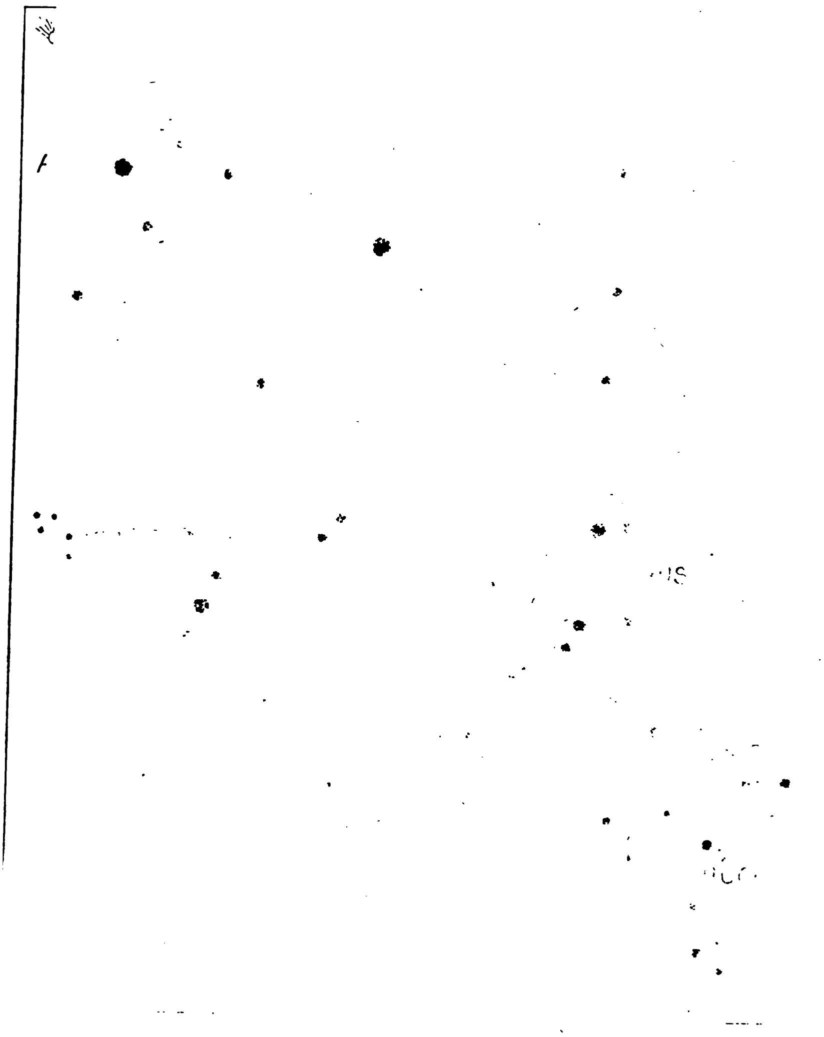

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXV. OCTOBER, 1902. No. 323.

Meeting of the British Association at Belfast.—Subsection: Astronomy and Cosmical Physics. Extracts from the Address by Prof. A. SCHUSTER, Chairman of Subsection.

OUR proceedings to-day constitute an innovation and require a few words of explanation. When, a few years ago, some astronomers felt that our Association bestowed an insufficient share of attention on their subject, an easy remedy suggested itself in the formation of a special subsection devoted to that subject. Such a subsection was accordingly organized at Bradford and Glasgow, but, for reasons which are perhaps not altogether to be regretted, the experiment was only partially successful. In the meantime the work of Section A became heavier and heavier, and, as it seemed necessary to find some way of relieving its meetings, it was decided to hand over to the already established subsection of Astronomy, other subjects, such as Meteorology, Terrestrial Magnetism, Seismology, and, in fact, anything that the majority of physicists are only too glad to ignore.

When the Council of the British Association asked me to act as President of such an enlarged subsection, I was very doubtful whether I ought to accept the honour. In the first place, I felt incompetent, owing to my almost complete ignorance of most branches of astronomy, and in the second place I do not approve of the formation of subsections dealing with important branches of Physics. If I eventually consented, it was partly because I lacked the strength of mind to refuse an honour of this kind, but partly because I was glad to have an opportunity of raising the whole question of the organization of our meetings. The ground for such a discussion has, however, to a great extent disappeared, because, when the Organizing Committee of Section A met in the spring there appeared amongst those present a sudden revival of interest in the subjects assigned to the subsection, and it was decided that the main section should not meet at all to-day so as to allow its members to help us in our discussions. The parent section has, therefore, voluntarily submitted itself to absorption by its neglected offspring, which now has to show that Cosmical Physics obeys the laws of Terrestrial Physics, and that good absorbers are also good radiators.

. . . .

The question I wish to bring to your notice to-day is an old one: if two events happen simultaneously or one follows the other at a short interval of time, does this give us any reason to suppose that these two events are connected with each other, both being due to the same cause, or one being the cause of the other? Everyone admits that the simple concurrence of events proves nothing, but if the same combination recurs sufficiently often we may reasonably conclude that there is a real connection. The question to be decided in each case is what is "sufficient" and what is "reasonable." Here we must draw a distinction between experiment and observation. We often think it sufficient to repeat an experiment three or four times to establish a certain fact, but with meteorological observations the case is different, and it would, *e. g.*, prove very little if on four successive full moons the rainfall had been exceptionally high or exceptionally low. The cause of the difference lies in the fact that in an experiment we can control to a great extent all the circumstances on which the result depends, and we are generally right in assuming that an experiment which gives a certain result on three successive days will do so always. But even this sometimes depends on the fact that the apparatus is not disturbed, and that the housemaid has not come in to dust the room. This may be difficult to secure in a laboratory, but it is possible. Here lies the difference. In the upper regions of the atmosphere it is impossible, and we are never sure whether some unseen hand has not made a clean sweep of some important condition.

When we cannot control accessory circumstances we must eliminate them by properly combining the observations and increasing their number. The advantage does not lie altogether on the side of experiment, because the very identity of condition under which the experiment is performed gives rise to systematic errors, which Nature eliminates for us in the observational sciences. In the latter also the great variety in the combinations which offer themselves allows us to apply the calculus of probability, so that in any conclusion we draw we can form an idea of the chance that we are wrong. Astronomers are in the habit of giving the value of the "probable error" in the publication of their observations. Meteorologists have not adopted this custom, and yet their science lends itself more readily than any other to the evaluation of the deviations from the mean result, on which the determination of the probable error depends. We look forward to the time when weather forecasts will be accompanied by a statement of the odds that the prediction will be fulfilled.

The calculation of the probability that any relationship we may trace in different phenomena indicates a real connection, seems to me to be vital to the true progress of Meteorology, and although I have on previous occasions drawn attention to this matter, I should like once more to lay stress on it.

The particular case I wish to discuss (though the methods are not restricted to this case) is that in which one of the two series of events between which relationship is to be established has a definite period, and it is desired to investigate the evidence of an equal period in the other series.

As an instance we may take the investigation of a connection between the Moon and earthquakes, or between sun-spots and rainfall. The question to be decided in these cases would be: Is there a lunar period of earthquakes or an eleven years' sun-spot period of rainfall?

Everyone familiar with Fourier's analysis knows that there is a lunar or sun-spot, or any other period in any set of events from volcanic eruptions down to the birth-rate of mice; what we want to find out is whether the periodicity indicates a real connection or not.

. . . .

A lunar effect on earthquakes is in itself not improbable, as we may imagine the final catastrophe to be started by some tidal deformation of the Earth's crust. The occurrence of over 7000 earthquakes in Japan has been very carefully tabulated by Mr. Knott according to lunar hours, who found the Fourier coefficient for the lunar day and its three first sub-multiples to be 10·3, 17·9, 10·9, 3·97; the expectancy on the hypothesis of chance distribution for these coefficients I find to be 19·3, 15·7, 10·6, 5·02. The comparison of their numbers disproves the supposed connection. On the other hand, the investigations of Mr. Davison on solar influence have led to a result much in favour of such influence; the amplitude found being in one series of observations equal to five times, and in the other to fifteen times the expectancy. The probability that so large an amplitude is due to accident in the first case is one in 300 millions, and in the second the probability of chance coincidence would be represented by a fraction, which would contain a number of over 70 in the denominator. We may, therefore, take it to be established that the frequency of earthquakes depends on the time of year. With not quite the same amount of certainty, but still with considerable probability, it has also been shown that earthquake shocks show a preference for the hours between 9 A.M. and noon.

A great advantage of the scientific treatment of periodical occurrences lies in the fact that we may determine *à priori* how many events it is necessary to take into account in order to prove an effect of given magnitude. Let us agree, for instance, that we are satisfied with a probability of a million to one as giving us reasonable security against a chance coincidence. Let there be a periodic effect of such a nature that the ratio of the occurrence at the time of maximum to that at the time of minimum shall be as $1+\lambda$ to 1, then the number of observations to establish such an effect is given by the equation $n=8m^2/\lambda^2$.

If there are one per cent. more occurrences at the time of maximum than at the time of minimum, $\lambda=\cdot01$, and n is equal to two millions. If the effect is 5 per cent., the number of events required to establish it is 80,000.

To illustrate these results, I take as a further example a suggested connection between the occurrence of thunderstorms and the relative position of Sun and Moon. Among the various statistical investigations which have been made on this point, that of Mr. McDowall lends itself most easily to treatment by the theory of probability. One hundred and eighty-two thunderstorms observed at Greenwich during a period of 14 years have been plotted by Mr. McDowall as distributed through the different phases of the Moon, and seem to show a striking connection. I have calculated the principal Fourier coefficient from the data supplied, and find that it indicates a lunar periodicity, giving for the ratio of the number of thunderstorms near New Moon to that near Full Moon the fraction 8·17 to 4·83.

This apparently indicates a very strong effect, but the inequality is only twice as great as that we should expect if thunderstorms were distributed quite at random over the month, and the probability of a true connection is only about 20 to 1. No decisive conclusion can be founded on this, the number of thunderstorms taken into account being far too small. We might dismiss as equally inconclusive most of the other researches published on the subject were it not for a remarkable agreement among them, that a larger number of storms occur near New Moon than near Full Moon.

. . . .

I believe that the curve which connects the intensity with the period will play an important role in meteorology. It is a curve which ought to have a name, and for want of a better one I have suggested that of periodograph. To take once more barometric variations as an example, it is easy to see that just as in the case of white light the periodograph would be zero for very short, and probably also for very long periods. There must be some period for which intensity of variation is a maximum. Where is that maximum? And does it vary according to locality? The answer to these questions might give us valuable information on the difference of climate. Once the periodograph has been obtained, the question of testing the reality of any special periodicity is an extremely simple one. If h be the height of the periodograph, the probability that, during the time interval chosen, the square of the Fourier coefficient should exceed kh is e^{-kh}. If we wish this quantity to be less than a million, *k* must be about 11; so that in order to be reasonably certain that any periodicity indicates the existence of a truly homogeneous variation, the square of the Fourier coefficient found should not be less than 11 times the corresponding ordinate of a periodograph.

I have calculated in detail the periodograph of the changes of magnetic declination at Greewich, taking as basis the observations published for the 25 years, 1871–95. It was not perhaps a very good example to choose, on account of the complications introduced by the secular variation, but my object was to test the very persistent assertions that have been made as to the reality of periodic changes of 26 days or thereabouts. The first suggestion of such a period came from Hornstein, of Prague, who ascribed the cause of the period to the time of revolution of the Sun round its axis. He only discussed the records for one year's observations, but the evidence he offered was sufficient to impress Clerk Maxwell with its genuineness. Since Hornstein's first attempts a great many rough and some very elaborate efforts have been made by himself and others to prove a similar period in various meteorological variations. The period found by different computors differed, but there is a good deal of latitude allowed if the rotation of the Sun really has an effect on terrestrial phenomena, because the angular velocity of the visible solar surface varies with the latitude. Hornstein himself and some of his followers deduced a period not differing much from 26 days, while Prof. Frank Bigelow, using a large quantity of material, finds 26·68 days, and Eckholm and Arrhenius return to 26 days, or, as they put it more accurately, to 28·929 days. The two latter investigators do not, however, adopt the idea that this periodicity is due to the rotation of the Sun. None of these periods can stand the test of accurate investigation.

As the result of my calculations I can definitely state that the magnetic declination at Greenwich shows no period between 25·5 and 27·5 days having an amplitude as great as 6″ of arc. The influence of solar rotation on magnetic variation may therefore be considered to be definitely disproved.

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The subjects with which our subsection is concerned deal with facts which are revealed to us by observation more frequently than by experiment. There is in consequence a very real danger that the importance of observation misleads us into mistaking the means for the end, as if observation alone could add anything to our knowledge. An observation made for its own sake, and not for some definite scientific object, is a useless observation; perhaps worse than useless, because it encumbers the shelving space in our brains, which is no more unlimited than that in our libraries. Science is not a museum for the storage of disconnected facts and the amusement of the collecting enthusiast. I dislike the name "observatory" for the astronomical workshop, for the same reason that I should dislike my body to be called a food receptacle. Your observing dome would be useless without your computing room and your study. What you want is an Astronomical Laboratory,—a Meteorological or Magnetic Laboratory, attaching to the word "laboratory" its true meaning, which is a workshop in which eyes and hands and brains unite in producing a combined result.

The problems which confront the astronomer being more definite than those of Meteorology, Astronomy has grown under the stimulus of a healthy tradition. Hence it it generally recognized, at any rate in the principal observatories, that the advance of knowledge is the chief function of the observer. Nevertheless, the President of the Astronomical Department of Section A last year (Professor H. H. Turner) has found it necessary, in his admirable address, to warn us against the danger there is, that the astronomer should allow himself to be swallowed up in a routine work and mere drudgery.

The descent is easy: You begin by being a scientific man, you become an observer, then a machine, and finally—if all goes well—you design a new eyepiece.

If such a danger exists in Astronomy, what shall we say about Meteorology? That science is bred on routine, and drudgery is often its highest ambition. The heavens may fall in, but the wet bulb must be read. Observations are essential, but though you may never be able to observe enough, I think you can observe too much. I do not forget the advances which Meteorology has made in recent years; but if you look at these advances, I think you will find that most of them do not depend on the accumulation of a vast quantity of material. The progress in some cases has come through theory, as in the applications of Thermo-dynamics, or through special experiments, as by kite and balloon observations, and when it has come through the ordinary channels of observation, only a comparatively short period of time has been utilized. It would not be a great exaggeration to say that Meteorology has advanced in spite of the observations and not because of them.

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Most earnestly do I believe that the subjects of Meteorology and Terrestrial Magnetism, and possibly also of Atmospheric Electricity, could be most quickly advanced at the present moment, if all observations were stopped for five years, and all the energy of all observers and computors concentrated on the discussion of the results obtained, and the preparation of an improved scheme of observation for the future. When we have made up our minds what to do with the observations, when we have actually done it; when we know where our present instruments require refining or supplementing, and especially when we have found out whether we have not spent much time and trouble on unnecessary detail, then the time will have arrived for us to draw up an economical, sufficient, and efficient scheme of observations. At present we are disinclined to discontinue observations, though recognized as useless, for fear of causing a break. We make ourselves slaves to so-called "continuity," which is important, but, may be, and I believe is being, too dearly purchased.

There are no doubt some, though probably not very many, observations which it is necessary to carry on continuously over long periods of time. But at present we are groping in the dark, and go on observing everything, and always in the hope that some time the observations might prove useful. Our whole point of view in this respect wants altering. We should fix on our problem first and then provide the observations which are necessary for the solution of the problem. Let us restrict, in the first instance, the secular observations to the smallest number, and concentrate our attention, for short periods of time, on some special question. Let us have, for instance, two or three years of thunderstorm observations, all countries joining in concentrating their energies to the elucidation of all the various features of their phenomena. When that is accomplished it will probably be found that thunderstorms may be left to shift for themselves for a while, and attention might be directed to some other matter. The whole question of lunar influence on Meteorological phenomena might be settled in a comparatively short space of time, if the civilized countries of the world could agree to record all observations during a few years according to lunar instead of solar co-ordinates. Other problems will readily suggest themselves to you, and several might possibly be dealt with simultaneously.

The great reform I have in view is this:—Before you observe, make sure that your observations will be useful and will help to answer a definite question.

I hope that, though my frankly outspoken criticisms may not command universal assent, you will agree that there is some foundation for them, and, if so, the time is obviously not well chosen when observational science can be separated from its mathematical and experimental sisters. We hope that Cosmical Physics may remain an integral portion of Section A, and, though we acknowledge our weaknesses, we claim to have also something to teach.

Meteor from a Radiant near Fomalhaut.

WHILE watching the sky on August 25, at $10^h\ 50^m$, a rather conspicuous meteor with a thick train of sparks crossed my zenith, traversing a path of 45° from 319° +40° to 256° +76°. It had a dull nucleus of perceptible diameter, though I rated it as only 2nd mag., and its duration of flight equalled about 7 seconds. The meteor sailed very slowly across the sky, and seemed to find great difficulty in struggling through the air.

Mr. C. L. Brook, of Meltham, near Huddersfield, also observed the meteor, and described it as a small, dull fireball moving slowly and increasing in brightness towards the end. Mag. 4 to 2 and duration 3½ seconds. Path 304° −16° to 287½° −8½°.

The two observations were made under very favourable circumstances, and show that the meteor was a belated member of a shower at 339° −26° near Fomalhaut in Piscis Australis. The heights of the object were from 65 to 57 miles from over 2 miles S. of Radstock, Somerset, to 5 miles E. of Hay, Hereford. Length of path 59 miles and velocity 11¼ miles per second from a mean of the two estimates of duration. The radiant was near the horizon in S.S.E. at the time, and the meteor is interesting as supplying further evidence of the fact that the long-pathed, very slow-moving meteors are almost invariably directed from radiants near the horizon in the S. or W. sky.

It would be useful to hear of further descriptions of the meteor, or of any of the following objects which have been selected from a number of recent observations obtained at Bristol and Meltham.

Observed at Bristol.

Date.	G.M.T.		Mag.	Path.				Duration.	Notes.	Assumed radiant.	
				From		To					
1902.	h	m		α °	δ °	α °	δ °	sec.		α. °	δ. °
Aug. 25 ...	11	15	♃	340	+40	13	+72½	3·5	V. v. slow.	323	−24
Sept. 1 ...	11	18	3	21	+52	358	+23	2·0	Slowish, streak.	127 140	+57 +49
3 ...	11	41	1	25	+64	327	+57	1·4	Swift, strk.	74	+41
5 ...	10	59	1	291	+65	286	+59	0·6	Swift	337	+82
„ ...	12	53	♃	290½	+52	232	+72	4·0	V. v. slow	323	−24
6 ...	10	28	2	358	+65	140	+86	2·0	Slowish.	353	−11
„ ...	11	34½	2×♃	12	+61½	340	+57¾	0·8	Swift, strk.	74	+41
7 ...	12	50	1	7	+52½	45	+42½	2·5	Slow.	290	+25
„ ...	13	38	3	108	+52	118	+69	0·7	Swift, strk.	100	+13

Observed at Meltham, near Huddersfield.

Date.	G.M.T.		Mag.	Path.				Duration.	Notes.	Assumed radiant.
				From		To				
1902.	h	m		α °	δ °	α °	δ °	sec.		
July 28 ...	10	25½	1	58	+38½	68	+44½	1·5	Swift.	Aries or Cetus.
29 ...	12	16	1	332½	+ 1½	330½	+ 8½	...	Swift.	337°−11°
Aug. 10 ...	10	50	1	265	+56½	202½	+ 50	3·5	Yellow; train.	Aquarius.
„ ...	11	7½	1	303	−11½	325	−15	3·0	Slow.	220°+12°
25 ...	10	28	2	350	± 0	12½	− 5	4·5	Very slow.	220°+12°

Observations during the last few years, from about the middle of August to the middle of September, have proved that there are two well-defined southern showers in Capricornus and E. region of Aquarius, from the points 323° − 24° and 353° − 11°.

Bishopston, Bristol,
1902, Sept. 14.

W. F. Denning.

Solar Stars *.

Dr. Schuster, as everyone knows, thinks for himself, and thinks definitely. His conclusions, moreover, are based on exact knowledge, and he is accustomed to bring them, when they admit of it, to strict numerical trial. What he writes, then, must always meet with respectful attention, especially when the subject treated of is of such particular interest as that of the mutual relations of stellar families.

The status of solar stars cannot be determined offhand. A number of difficult and delicate questions have first to be answered, and their ramifications go deep down to the very foundations of astrophysical science. To trace them out in detail would need years of experiment and volumes of exposition. It must suffice for the present to recognize that they exist, and may, for anything we can tell, be of very wide import. Dr. Schuster, meanwhile, has adopted the promising course of seeking in the Sun a key to the riddles set by the stars. The Sun, at any rate, presents a broad disc, open to inspection; the stars are "atoms of intensest light," defying localized scrutiny. They present to us in shorthand what is written at full length on the solar page. If our enquiries are to be ordered aright, they must then begin with the fair text, although it convey meanings often hard to understand.

Dr. Schuster tabulates at page 12 of his reprinted paper some instructive results, calculated by himself, chiefly from Ritter's data, as to "the density, the pressure, and the temperature of a star having the same mass and size as the Sun; and behaving like a perfect gas in a state of convective equilibrium." And first we notice the "curious fact," which, as the author says, "is not perhaps without significance," that the computed central density "is only very little in excess of the density which, according to the most careful recent estimate, is to be ascribed to the central portion of the Earth, and again, that that estimate is very little in excess of the density of solid iron." The figure arrived at is 8·44 times the density of water; but it corresponds to the inconceivable, though far from unlikely, pressure of approximately 8500 billion atmospheres. Halfway to the surface the compactness of the solar globe nearly equals that of hematite, the pressure having diminished to less than one quarter its maximum value; while, forty-four thousand miles still higher up, or at a level about 260,000 miles from the centre, the compressed vapours and gases, under a weight of superincumbent strata equivalent to one-thousand billion atmospheres, possess the density of gypsum. All this is very much what might have been expected; but the accompanying temperatures pass the bounds of credibility. They depend, to some extent, upon the assumed nature of the substance or substances constituting the Sun, and the figures in the Table apply to hydrogen in its ordinary diatomic state. Its temperature at the centre of the solar globe comes out 24 million degrees centigrade; while if monatomic iron be substituted, the estimate has to be raised to 672 million degrees! Thermal arithmetic breaks down long before this transcendental pitch is attained. Such numbers are without meaning; they stand for no facts; they convey no ideas. Indeed, they are given only as illustrating the failure of the process by which they were arrived at—a failure due, as Dr. Schuster expressly mentions, to the incompleteness of the data serving as the basis of his equations, which included no allowance for effects of radiation, conduction, or diminishing compressibility towards the centre.

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* 'The Evolution of Solar Stars.' By Prof. Arthur Schuster, F.R.S. Proceedings Royal Philosophical Society of Glasgow, 6 November, 1901.

His discussion of convective action within the body of the Sun is all the more important because of the inadequate attention sometimes paid to that leading trait of the solar economy. Thus, he shows that, since the whole of the heat contained in a superficial layer 370 metres thick is radiated away into space in each second of time, this shell of exhausted matter has to be replaced, second by second, from the interior, if the thermal outflow is to be maintained; and he computes at one thousand miles an hour the velocity of the currents by which the exchange is effected, allowing a pressure-difference of one atmosphere between the ascending and descending flows: unless, that is to say, an appreciable portion of the heat be supplied by some such means as interior radiation. This seems unlikely, but the possibility is held by our author in reserve. What remains certain is that the imperative necessity for the transport of heated materials from within outward determines the mode of circulation in the vast organism which has for us literally a vital interest. What is true of the Sun is true of the stars. They necessarily radiate more intensely in proportion to the rapidity with which stores of energy can be brought to the surface; and this augments with the effectiveness of gravity, but slackens with the growth of viscosity. The extraordinary superficial lustre belonging certainly to helium and hydrogen stars, and probably to giant stars of the solar type, like Capella and Canopus, shows conclusively that, in the more primitive globes as well as in some with a more "advanced" spectrum, the balance of conditions strongly favours sub-photospheric mobility.

On the thorny subject of relative stellar temperatures, Dr. Schuster makes some useful remarks. Indefiniteness of phraseology here, as in many other cases, has tended, he says with truth, to confuse the issue. The validity of "Lane's Law," that gaseous bodies grow hotter as they contract, cannot be tested by observations from the outside. And only observations from the outside are possible to us. "We do not," he continues, "observe the temperature of the centre, or the temperature of the gaseous mass below the photosphere, and it is with this temperature that the theoretical analysis deals. What we can observe is the photosphere and the absorbing layer above it, and the temperatures of these portions of the Sun are not touched by Lane's theory.....The surface of a star is pouring out energy in the form of radiation, and the temperature of the surface will depend on the balance of a number of delicately poised conditions."

That it is higher for Sirian than for solar stars, he considers to be proved by the results of Professor Nichols's experiments on stellar heat-radiation, lately carried out with the utmost care at the Yerkes Observatory. They indicated the possession by Arcturus of double the heat-power of Vega, although we receive from each object approximately the same amount of light. The obvious inference is drawn by Dr. Schuster that the maximum of energy being situated considerably lower down in the Arcturian than in the Vegan spectrum, it must be derived from a proportionately cooler source. But when we take into account the respective effects of absorption in the atmospheres of the two stars, we find the grounds for the inference to disappear; for in solar stars the shortest wave-lengths are predominantly arrested, while in Sirian stars they issue forth almost unimpeded. A reddish veil covers the discs of the former class of objects. Its removal would at once give a great extension to their ultra-violet spectra, and cause them to show the steely glitter of Golconda brilliants instead of the "off-colour" assimilating them as they are rather to Cape diamonds. Under these impossible circumstances the comparison of the relative heat- and light-strength of Vega and Arcturus would be legitimate and might be instructive; as things actually are, it is one-sided and misleading.

Direct methods thus fail to breach the walls of this problem, but indirect approaches to them have been effected not altogether without success. Especially, the confrontations instituted by Sir Norman Lockyer and Dr. Scheiner of the spectral lines most prominent severally in Sirian and solar stars have made it clear that the prevalent conditions in the reversing-layers of the first kind approximate to those present in the electric spark, while the arc reproduces quite closely the state of the vapours causing the familiar Fraunhofer-absorption. But whether differences of temperature are the genuine equivalents of the observed varieties in the mode of luminous excitement, is a point which becomes, year by year, more arduous of decision.

The usual lines of thought are followed, in the discussion we are now considering, as regards the successive development of helium, hydrogen, and solar stars. Their diversities are, however, explained on quite novel principles. The crux of the matter resides, as Dr. Schuster notes with his customary acumen, in the gradual thinning-out of the hydrogen-lines *pari passu* with the widening of the calcium-lines. Sir William and Lady Huggins connect this progressive exchange of intensities with the slow increase in the power of surface-gravity incidental to the advance of condensation. Our present author, who, by the way, admits only provisionally the calcium origin of H and K, believes that hydrogen is actually *soaked up* by ageing globes, and so tends to disappear from their atmospheres. And helium goes by the same road, but sooner. Changes in the quantity of material present are then assumed for the purpose of accounting for spectral varieties. Nevertheless, we have tangible proof that, in the Sun at all events, prodigious volumes of both hydrogen and helium continue to exist. An envelope mainly composed of them, five thousand miles in depth, should certainly suffice, apart from countervailing influences, to produce any conceivable spectroscopic effect. That, in their absence, it would indeed do so, is demonstrated by the occasional emergence, in the penumbræ of sun-spots, of strong helium-absorption. By a passing disturbance in the Sun, accordingly, the normal condition, in this respect, of helium-stars is visibly evoked. Most probably, the darkening of D_3 and λ 6678—thrice recorded by Father Cortie in 1883—testifies to an escape of helium from the interior due to relief of pressure in the spotted neighbourhood. But, in any case, the fact is most significant that the released gas is in a condition to absorb, while chromospheric helium is non-absorbent. Further, the abrupt spectral changes of certain comets during their approach towards and withdrawal from the Sun, the blazing of hydrogen and helium in long-period variables near maximum—above all, the replacement of stellar by nebular rays in waning temporary stars,—leave no doubt that substances need not be removed to become non-apparent. The chemical constitution of Novæ remains the same throughout the multiplied alterations of their light-vesture. What does *not* remain the same is evidently the mode of their illumination. And Professor Dewar, in his Belfast Address, made a demand that will surely be complied with, when he called for a revision of theories attributing the "more permanent differences between the spectra of different stars to differences of temperature, and a fuller consideration of the question whether they cannot with better reason be explained by differences in the electric conditions which prevail in stellar atmospheres."

Dr. Schuster holds gaseous nebulæ to represent the tenuous leavings of primordial aggregationary processes. By the feeble gravitative pull of inchoate stars, neither hydrogen nor helium could be held under control. They were hence at liberty to diffuse through boundless fields of space, where they may still be seen to glow at large. They have, however, been partially recaptured, since the co-existent, condensing masses of matter acquired by degrees power to annex them in the form of atmospheres, more or less extensive according to the copiousness of the gaseous supplies at hand. Stellar composition may then, in our author's opinion, vary with the local chemistry of space. All of which may, or may not, be true. The far background of time is lit only by fitful glimmers, quite possibly of the will-o'-the-wisp kind.

A paper replete with original suggestions concludes with a telling protest against the indiscriminate application of the doctrine of uniformity, "which has always," the writer says, "proved a fallacious guide." Had such a law, he continues, "reigned at creation, there could have been no life, for there can only be uniformity in death; but if there were sufficient diversity of position, of mixture, and composition, to allow of aggregations of matter culminating in the formation of worlds, we may be sure that we shall be able to trace that diversity in the present composition of the stellar system. The universe shows law, order, and regularity, but it refuses to be forced from birth to death through a single channel. There is uniformity no doubt, but it is a uniformity which, at all times, and in all places, is relieved by endless variety." AGNES M. CLERKE.

The Advance of Astronomy[*].

"NOT to know me," says Milton, "argues yourselves unknown," and upon a cognate principle it may well be affirmed that to be ignorant of Miss Clerke's admirably complete compendium of the history of the astronomy of the last hundred years is practically equivalent to the forfeiture of any legitimate claim to be considered an astronomer at all. Inasmuch, however, as seventeen years have elapsed since the first edition of Miss Clerke's now classical work appeared, and that it is nine years since the third edition was given to the public, it may not be uninstructive to review some of the additions to our knowledge which have been made during this interval, and to take some note of the directions in which it has been made.

In limine, then, let us see how far research into the physical constitution of the Sun has been successful since the year 1893, the date of the previous edition of the work now before us; and first with reference to the structure of sun-spots. Now from the time of Alexander Wilson in 1769, until within a relatively recent period, it has been almost an article of faith that the ordinary sun-spot consists of a funnel-shaped depression in the solar photosphere, the shelving sides of which we see as the penumbra, while the cooler internal nucleus affects the eye as a dark spot. Nay, Sir William Herschel went so far as to surmise that the dark portion of the sun-spot was actually the comparatively cool and solid nucleus or body of the Sun itself, and that in all probability that was inhabited! Thanks to the persistent attention which has of late years been paid to solar phenomena, all this is now known to be utterly fallacious, and the mighty orb which rules our terrestrial day must be more accurately described (as it was by the late Richard A. Proctor) as "a bubble" than as a body in any sense possessed of solidity as we understand that term. Furthermore, the sequent and protracted observations of the Rev. F. Howlett, extending over a period of between thirty and forty years, coupled with those made at the Stonyhurst Observatory, have thrown grave doubts on the assumption that sun-spots are depressions at all. Very valuable advances, too, have been made in tracing the connection between sun-spots and faculæ, which Professor Hale has succeeded in photographing over the entire solar surface. Nor has our knowledge of the chemical constitution of the Sun's surroundings remained stationary. Up to 1895 no terrestrial source of the familiar orange-yellow ray in the prominences was known, but in that year Professor Ramsay discovered a volatile gas in the mineral known as clevite, whose spectrum included this ray, together with certain others found by Professor Young in the envelope immediately surrounding the photosphere. Two notable lines of calcium which are conspicuous near the visible limit of the chromospheric spectrum, while others were absolutely imperceptible, were shown in 1897 by Sir William and Lady Huggins to owe their apparent isolation to the extreme tenuity of the emitted vapour. In this same year Professor Hale discovered a low-lying stratum of carbon vapour in the chromosphere; and while in 1893 Starkey detected in the eclipse photographs the presence of the rare metal gallium, Evershed's photographs taken in 1898 further revealed that it was accompanied by the equally rare metal scandium, while Runge and Paschen in 1896 identified three dark lines low down in the red, which were shown in photographs taken by Higgs and McClean, with the fundamental group of an oxygen series. To pursue this fascinating subject, however, would be to occupy our limited space to the exclusion of others of the highest interest, so we must pass to the consideration of another branch of our subject, and one of the most capital importance—that of the Sun's distance. It is a very familiar fact that up to a comparatively recent period the Transits of Venus were relied upon as furnishing the most readily available and reliable means of determining this constant, or that from which it is immediately derived, the solar parallax. The experience gained, however, in 1874 and 1882 sufficed to show the comparative untrustworthiness of this method, for while Houzeau deduced a parallax of 8″·907 (which would show the Sun's mean distance from the Earth to be only 91,727,000 miles), the English results increased that distance to 92,560,000 miles, reduced by Professor Harkness's measures of photographs to 92,360,000, and still further reduced by Auwers' deduction of 92,000,000 from the German heliometric measures. A theoretically perfect method, that of a combination of the constant of aberration with Newcomb's superb determination of the velocity of light, loses some of its value from the variation in that constant as obtained by very high authorities. Meanwhile, a parallax of 8″·80, and a constant of aberration of 20″·47, has been adopted in all the National Ephemerides. In 1898, however, a discovery was made of a minute member of the almost innumerable cluster of planetoids revolving between Mars and Jupiter, which has since received the name of Eros, whose orbit is of such eccentricity as to bring it once in every thirty years within 15,000,000 miles of the Earth. A fairly favourable opposition occurred in 1900, and no less than 58 observatories joined in the task of photographing Eros and its vicinity on that occasion, a task performed with the most complete success. As we write, the reductions are in full progress, and we ought not to have long to wait for a result of the highest value and interest.

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[*] 'The History of Astronomy during the XIXth Century.' By Agnes M. Clerke. Fourth Edition. London: Adam & Chas. Black, 1902. 15s. net.

The discoveries made in connection with the planets and their satellites since the appearance of the third edition of the work under review need not detain us long. Schiaparelli's allegation that Mercury and Venus rotate but once on their axes during the period that they occupy in revolving round the Sun, though apparently corroborated by some competent observers, has been strongly contested by others of equal eminence. Miss Clerke opines that "Schiaparelli's period of 225 days (for Venus) must be allowed to hold the field." For ourselves we should prefer the Scotch form of verdict: "Not proven." Viewed as a planet, the work on our own Earth may be dismissed in a very few lines. The geodesical observations at the Cape will be immediately resumed, and the great Indian arc is being steadily lengthened. Within the last three weeks a most important paper has been read before the British Association at Belfast, by Major S. G. Burrard, R.E., in which he insists on the uncertainty introduced into the determination of latitude by the deflection of the plumb-line. This is a question which demands, as it will assuredly receive, the most rigid investigation and attention. Recent observations by more than one of our leading selenographers seem to point to visible evidence of changes in the Moon still in progress. Of Mars it will be enough to say that "canals" and cognate phenomena have been discovered at an alarming rate during the last few years. The Dutch, to use an expressive vulgarism, apparently "aren't in it" with the Martians in this respect. Photography is responsible for numerous annual additions to the mass of orbicles revolving between the orbits of Mars and Jupiter. Something like 500 of these pocket planets are now catalogued. Of the, to us, most important of them all, Eros, we have spoken in another connection above. The most important discovery in connection with Jupiter has been that of a fifth satellite, first glimpsed by Professor Barnard in the 36-inch Lick Refractor, on September 9, 1892. If we can depend upon the observations of Messrs. W. H. Pickering and Douglass, Jupiter's IIIrd and IVth satellites rotate on their axes in the same time that they take to revolve round their primary, just as our own Moon does; while the rotation of the Ist and IInd are distinctly swift. This furnishes a curious dynamical puzzle. The supposed discovery of a ninth satellite to Saturn has not been corroborated, while, finally, the additions to our knowledge of Uranus and Neptune have been practically nil. Professor George Forbes, of Edinburgh, believes that yet another planet exists in the depths of space whose orbit is exterior to that of Neptune; but the most careful scrutiny of the region of the sky in which it should be visible (assuming it to exist at all) has failed to reveal it. But scant addition to our knowledge of the nature and constitution of comets is creditable to recent years.

It is to the photographic plate and the spectroscope that we owe such recent advances as have been made in our knowledge of the stellar heavens. The 'Draper Catalogue' of the renowned Harvard Observatory must ever remain as an unsurpassed record of the chemical constitution of the mysterious bodies which spangle the celestial vault—revealing as it does, in effect, the whole process of stellar evolution. The first volume, published in 1890, includes 10,351 stars down to about the 8th magnitude, comprised between the North Pole and 25° of South Declination. The second volume, soon to appear, will comprise no less than 30,000 stars in both hemispheres. Supplementing this splendid work two important detailed discussions of stellar spectra have been published—the first in 1897 by Miss A. C. Mason, dealing with 681 bright stars visible in the northern hemisphere; the second by Miss A. J. Cannon, in 1901, with 1122 southern stars. In the discussion of stellar spectra discoveries of the highest interest have been made. Chief, perhaps, among them may be mentioned that by the shifting of certain lines it has been shown that several stars which appear absolutely single as viewed in the gigantic telescopes now constructed, indicates that they are in reality excessively close binaries, whose orbital motion in the line of sight causes them alternately to approach and recede from the observer. Among the more familiar stars whose duplicity has been detected by this exquisitely delicate method of research may be mentioned Mizar, β Aurigæ, Spica Virginis, Capella, and Polaris. For details of all that is known of the so-called "Novæ"—Nova Aurigæ, Nova Persei, &c.—the 'History of Astronomy' itself must be consulted and carefully studied.

The structural unity of the stars and nebulæ has been as beautifully as convincingly shown by the perfectly admirable photographs taken by Dr. Isaac Roberts, F.R.S., in which, as on a diagram, we can trace the evolution of systems, from the first cycling of stupendous masses of luminous vapour, through nebulous stars, following their spiral contour, to the fully developed suns of the depths of space. Of the universally known scheme of a general photographic survey of the heavens, the bare mention here will suffice. It is still in progress, and when complete must ever form an epoch in astronomical research.

But waning space warns us that we must conclude. There are numerous points in the recent advances in Astronomy on which we would fain have touched, but we trust that we have given some idea—we fear more or less faint—of the wealth of information which awaits the possessor of this last edition of a book whose charm of style is only equalled by its evidence of erudition and the value of its contents. We read in catalogues of books "which no gentleman's library should be without." Emphatically no astronomer's library is entitled to be considered complete from whose shelves Miss Clerke's 'History of Astronomy during the Nineteenth Century' is absent. WILLIAM NOBLE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

William Ball and Saturn's Ring.

GENTLEMEN,—

In Prof. Young's 'Text-Book of General Astronomy (Revised Edition, 1898, p. 392), alluding to the idea (which I had some share in disproving through your pages) that Ball anticipated Cassini in the discovery of the duplicity of Saturn's ring, he adds in a footnote that the drawing (which had been omitted in many copies of the *Philosophical Transactions*), when found, showed "that he [Ball] did not see the division at all, nor indeed even understand that the appendage was a ring."

When writing this, Prof. Young must have overlooked the passage in Huygens's 'Brevis Assertio,' which I quoted in the fifth volume of the *Observatory*, p. 334. It is unnecessary to give that again here, but I will quote two passages in the correspondence of Huygens, which has recently been published by the Société Hollandaise des Sciences. What led to the discovery of the true nature of Saturn's ring, and that it did not consist of two excrescences on either side of the planet, as appeared to Galileo, was the perception of a faint dark line or shadow crossing the ball of the planet, and this Ball did notice. The first quotation is from Wallis to Huygens (vol. ii. p. 305), which runs thus:—"Saturni fasciam, quam et te vidisse insinuas, observavit (quantum scio) primus ante aliquot annos Dominus Gulielmus Ball, Domini Petri Ball Equitis filius (frater illi quem tibi nuper commendatum deducam)"; and the second in one from Wren to Neile

('Correspondance,' vol. iii. p. 422), which is as follows:—"Conversionem hanc Corporis Saturnini (qualem in nostrâ Hypothesi fieri statuimus) Balthei Saturnini Apparentia fortasse confirmat; hunc tribus ferè abhinc annis primus conspexit Vir Illustris Gulielmus Ball, et nobis protenus ostendit."

These letters were written after Huygens's famous anagram, but before the appearance of his 'Systema Saturnium'; and in his 'Brevis Assertio' (published in 1660 in reply to the animadversions made upon that work) he refers to the observations of Ball as tending to the same conclusion as his own, the English observer having also recognized the ring-like form of the appendage to Saturn.

With regard to the supposition that Ball somewhat later noticed the duplicity of the ring, it must be remembered that this conjecture (it was no more) was made by Sir Robert Moray from a complete misunderstanding of a drawing by Ball, the cause of which was comparatively recently explained by Adams. It had got doubled in a peculiar manner, and the duplicity surmised by Moray was not that of two concentric rings one within the other, but of two rings about the same size, the planes of which formed an acute angle with each other. Yours faithfully,

Blackheath, 1902, Sept. 5. W. T. LYNN.

Benjamin Banneker, the Negro Astronomer.

GENTLEMEN,—

In the *Observatory* for August (vol. xxv. p. 304), Rev. S. J. Johnson has called attention to the negro astronomer, Benjamin Banneker, who published almanacs for the years 1792–1802 in the United States. He states that Banneker died in 1809. The *Atlantic Monthly* (vol. xi. p. 79), which may otherwise have been his source of information, gives this date as 1804, though the entire article in this *Monthly* is a tissue of misstatements. Allen's 'American Biographical Dictionary' says he died in October 1806, aged 70, and in at least one other publication the year is given as 1807. An obituary notice was printed in the *Federal Gazette and Baltimore Daily Advertiser* of Tuesday, 28 October, 1806, and this states that his death occurred on Sunday, 9 October. Since the 9th was not a Sunday, this is surely a misprint for 19 October, 1806, which must have been the date of his death, at the age of 75, since the family Bible which his parents possessed records his birth as being "November the 9th day, in the year of the Lord God, 1731." Yet one publication says he was born in 1734, and the *Atlantic Monthly* says 1732, while Allen's "aged 70" would make it 1736. This is pointed out rather more as an illustration of the looseness with which such dates are quoted than because of the intrinsic importance of these particular dates.

The Rev. Mr. Johnson also says: "His father was taken as a slave in Africa and sold in America. His mother was the child of natives of Africa." As a matter of fact, knowledge of his ancestry is not wholly obscure. His maternal grandmother was a white woman "of remarkably fair complexion," Molly Welsh by name, a native of England, who was deported to America when convicted of the theft of a bucket of milk which she claimed the cow had kicked over! On her arrival in this British Colony she was sold into servitude for a term of seven years to defray the expenses of transportation, as was the custom of the time. At the expiration of this period she purchased a tract of land for a mere trifle in the new country, and secured two negroes from a recently arrived slave-ship. One of these, who claimed he was the son of a chief in Africa, was named Banneker, as he called himself. After a few years, because he had such a noble bearing, though lazy disposition, she freed him and married him. Their oldest child, named Mary, also married a freed native of Africa, and he took *her* surname, Banneker. Their only son was this Benjamin Banneker, whose colour, however, as he himself states, was of the "deepest dye."

Benjamin Banneker had uncommonly soft and gentlemanly manners and pleasing colloquial powers. He was a pioneer in America in the movement towards improvement of his race. He was a farmer, owning his own land. He never married, and at the age of 60 sold his farm for a life-annuity for the sole purpose of enabling him to devote his unrestricted time to astronomical pursuits, in which again he was a pioneer in this country.

Besides the publication of his almanacs for ten years, his most creditable work was the assistance rendered Major Ellicott in surveying the District of Colombia and laying out the City of Washington. By his mental vigour and skill in calculations, as well as by the agreeableness of his personal qualities, he won the admiration and esteem of all his white associates in this task to such an extent that, notwithstanding the prevalence of slavery and the degradation of his race all the country over, he himself was treated as their equal and companion, not alone in the field, but also in the mess-room.

While Banneker always spelled his name in this manner, yet in some legal papers it is found as Banneky, and has been sometimes printed as Bannaker and Banniker.

Though he was probably the most adept of all his race in astronomical lore and mathematical attainment, he is not the only negro who even at that date cultivated this science. The most notable other example of whom I have information being Anthony George Amo, who was born in Guinea, West Africa, about 1703. When a child he was carried to Amsterdam, and presented to the Duke of Brunswick-Wolfenbüttel, Antoine Ulrich. The Duke gave the negro boy to his son, who sent him to the University of Halle to study. Amo there published his inaugural dissertation in 1729. Thence he went to the University of Wittemberg, and afterwards became Counsellor of State at Berlin. After the death of his protector, the Duke, he returned to Africa, where he lived the life of a hermit and had the reputation of a conjurer. Hebrew, Greek, Latin, French, German, and Dutch are said to have been equally familiar to him. He was met by Henry Gallaudet when travelling in Abyssinia in 1753, and he is supposed to have died shortly after this date.

To the Astronomers of England it may be of especial interest—recurring to the subject of almanacs—to read the following:—

"One of the most singular instances of punishment for an oversight was that shown by the commitment of an almanac-maker to the Bastile in 1717. It was made out by order of the Duke of Orleans, Regent during the minority of Louis XV. of France, and read as follows: 'Laurence d'Henry, for disrespect to King George I. in not mentioning him in his almanac as King of Great Britain.'"

International Latitude Observatory,
Gaithersberg, Maryland, U.S.A.,
1902, Sept. 8.

HERMAN S. DAVIS.

Goodricke and Pigott.

GENTLEMEN,—

The fact that John Goodricke was a deaf mute is, I find, mentioned in Robert Davies's 'Walks through the City of York' (edited after his death by his widow in 1880), p. 47, where are some interesting particulars about Goodricke *. He was educated at the academy at Warrington, and, notwithstanding his infirmity, made good progress both in classics and mathematics. His father resided in York, at Lendal, near the left bank of the Ouse, until his death in 1784, when his widow and her family, including John, continued there. Edward Pigott's house was at Bootham, which is about a quarter of a mile to the north-west of the Minster; in a small building (since pulled down, as we learn from Davies's 'Walks,' p. 10) in the garden behind the house he and his friend John Goodricke carried on together their astronomical observations. Pigott's last astronomical paper (on variable stars) is dated from Bath in August 1796 (*Phil. Trans.* for 1797, p. [133]); and it was there (as before stated) that he discovered (not knowing that he had been anticipated) the comet of 1807, and communicated the fact of its appearance to Herschel. A remarkable circumstance connected with that comet is the close similarity of the elements of its orbit to those of the third comet of 1881, although the period of each appears to be mainly, if not quite, two thousand years in length, so that they present a case of a cometary family. Nothing more is known of Pigott after his discovery of this comet in 1807, nor have I been able to ascertain the dates either of his birth or death. He was probably a younger son, as his brother, Charles Gregory Pigott, came into possession of the family estates.

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* There is, however, a blunder in p. 48, where it is stated that he lived till 1792, though the date is correctly given in p. 47 as 1786.

Whilst Goodricke was Dutch on the mother's side, Pigott was French, and, indeed, was probably born in France, as his father, Nathaniel Pigott, married a Caen lady, and resided there for several years before he returned to England. Edward Pigott was educated at Caen, and began his astronomical observations by assisting his father in his geodetical work in the Netherlands in 1773.

Yours faithfully,

Blackheath, 1902, Sept. 13. W. T. Lynn.

P.S.—Allow me to point out a misprint in my letter in your July number. At p. 272, line 20, "Dearham" should be "Durham."

The Red Spot Region of Jupiter.

Gentlemen,—

I quite fail to appreciate the points raised by Herr Leo Brenner in his letter on this subject. He says the following part of the large, irregular, dusky mass lying in the S. tropical zone of Jupiter has *nothing to do* with the preceding side, whereas it apparently forms part and parcel of the same object, and is undoubtedly due to developments arising from one and the same disturbance, or from a series of disturbances of precisely similar origin and character. Herr Brenner's identification of that portion which he calls "the real pyramid spot" is rendered doubtful by his own observations, for in 1901 he found it gain 48° in 85 days on System II. of the ephemerides, and the latitude of the *f.* end on October 12 was 213° (*Ast. Nach.* 3753). On 1902, August 13, he ascertained the longitude to be 75°, whereas it ought to have been about 42° (or 33° to the W.) according to the rate of motion at which it moved in 1901, for the velocity of the south temperate current, in which it participates, is very equable, and exhibits no marked variations such as affect other latitudes. Herr Brenner's observations of the *f.* end of this marking indicate a rotation-period of $9^{h}\ 55^{m}\ 17\frac{1}{2}^{s}$ in 1901, and $9^{h}\ 55^{m}\ 22^{s}$ for the ten months 1901 October 12 to 1902 August 13. The actual mean period, derived from a large collection of transits of the middle of the spot by various observers between 1901 June and 1902 September, is $9^{h}\ 55^{m}\ 18^{s}$.

On 1902, September 13, with a 10-inch reflector, power 312, I obtained the following observation of the spot:—

Preceding end on C.M.	$7^{h}\ 34^{m}$,	long. = 319°·3
Following end on C.M.	10 8	long. = 52·4

The length of the marking was therefore 93°, and the middle situated in long. 5°·8. This is really not far from the predicted place (12°·3) of the darker region of the object for September 17 (*B. A. A. Journal*, xii. p. 122, Jan. 1902).

During the 16 months this marking has remained visible it has doubtless encountered some vicissitudes, resulting in its visible extension both on the E. and W. sides. In a case like this it seems desirable to treat the object in its entirety, for physical changes occasion difficulties in identifying details of structure. Mr. Phillips, in his letter published in your August number, discussed the marking as a whole, and certainly adopted the best method possible under the conditions.

On September 13 the Red Spot was estimated central at $9^h\ 48^m$, so that its long. was 40°·3. This observation compared with one on April 28, when the long. was 45°·9, shows that the rotation period of the object has recently been $9^h\ 55^m\ 39^s$, which is nearly 3 seconds less than it was about 3 years ago.

The aspect of Jupiter's surface during the present apparition is unusually interesting and replete with detail. There are a number of white and dark equatorial spots, with a period almost coinciding with the rate ($9^h\ 50^m\ 30^s$) of System I. Various markings in the S. temp. region indicate a period of about $9^h\ 55^m\ 19^s$, the N. tropical spots, of which there are several conspicuous examples, $9^h\ 55^m\ 28^s$, and the N. temp. spots $9^h\ 55^m\ 57^s$. The observations are still in progress, however, and the rate of the different currents will be more accurately determined at the end of the year. During the past spring and summer seasons definition has been rarely good; the weather has been cloudy and ungenial, and generally unsuitable for observational work.

Bishopston, Bristol, Yours sincerely,
1902, Sept. 14. W. F. DENNING.

GENTLEMEN,—

I cannot at all agree with Herr Leo Brenner in his statement in the September issue of the *Observatory* to the effect that the dark south tropical spot seen by Mr. Denning and myself on the morning of June 28th last in about longitude 19° "has nothing to do with the interesting marking of the opposition of 1901." On the contrary, all the evidence appears to prove conclusively the opposite to be the case.

(1) Numerous observations obtained last year show that the spot soon began to extend longitudinally, and that this extension was continued to the end of the apparition.

(2) The rotation period determined from last year's observations would have brought the centre of the spot into conjunction with the Red Spot about the middle of July, so that there must have been a very marked alteration in its velocity if it were still on the *f.* side of the Red Spot (as Herr Brenner says it was) as late as August 13th.

(3) The dark spot seen in λ 19° on June 28th, as it receded from the Red Spot, has, without any break or interruption, been followed by more dark matter, so that the whole space between its *p.* end and the *p.* "shoulder," in spite of recent striations and

local variations in intensity, has presented the aspect of *one dark continuous mass.*

It seems to me that these facts prove conclusively that Herr Brenner is in error, and that the marking seen by me on June 20th and by Mr. Denning and myself on June 28th was without a doubt the *p.* end of the dark spot of last year, which had during the period of Jupiter's invisibility continued to spread out longitudinally both east and west. The only question raised which appears to me debatable is as regards the manner of the transference of this dark mass to the *p.* side of the Red Spot. Did it pass round or under the Red Spot? Owing to bad weather and very unfavourable seeing conditions my own observations have not been sufficiently numerous or satisfactory for me to speak definitely on the point, and I should be very glad to receive the observations and impressions of other observers. There is nothing of the long dark mass left now on the *f.* side of the Red Spot hollow, the dark wisp between the *f.* "shoulder" and the South Temperate Belt having been a more or less permanent feature for some time past. The brilliant white spot on the South Tropical Zone seemed, the last time I saw this region of the planet, to be shifting in latitude southwards, and to be approaching the angle formed by the South Temperate Belt and the dusky streak just mentioned at the south-east end of the Red Spot. This would appear to indicate that the interesting stream of matter which has given rise to this discussion is passing round the south side of the Red Spot *viâ* the South Temperate Belt. There is still a considerable quantity of dark matter lying south of the Red Spot, and it is important to follow this critically as well as the motion and behaviour of the bright spot I have just referred to. Unfortunately, I have been able to do very little observing of late owing to various causes, but I sincerely hope that others have been more favoured and more successful than myself.

The increased velocity of the Red Spot hollow referred to in my last letter has been maintained. The following are the latest C.M. transits I have secured of this object and the corresponding longitudes :—

	Transit Time. h m	°
July 27........	10 19	λ = 42·0
29........	11 58	42·6
Aug. 3........	11 4	42·0
15........	10 55·5	41·6
20........	10 4·5	42·6
27........	10 48	41·3
Sept. 3........	11 34	41·4
8........	10 38	39·1

Yours faithfully,

Croydon, 1902, Sept. 16. THEODORE E. R. PHILLIPS.

OBSERVATORIES.

[Continued from p. 338.]

Heidelberg. *Max Wolf.*—320 separate photographs were taken of various portions of the sky, with a total exposure of 590 hours. For work on small planets 376 plates were exposed, on which were 33 new planets and 105 old ones; the positions of 91 planets being measured on the plates. Work on variable stars, including photographs of Nova Persei. Attention was also given to the zodiacal light.

Jena (University Observatory). *Otto Knopf.*—Parallax of Eros. Observations of small planets. Meteorology.

Jena. *W. Winkler.*—Measures of double stars. Observations of Nova Persei. Observations of Sun on 240 days.

Kalocsa. *J. Fenyi, S.J.*—Observations of Sun's surface less interesting than in 1900; it was observed on the projection apparatus on 222 days, of which 176 were free from spots, the only large spot being the one appearing on east limb on May 18. The prominences were few and low; the Sun's limb was examined on 163 days completely, and 17 times partly, only 6 prominences were noted over 100″, the highest reaching 146″.

Kasan. *D. J. Dubiago.*—The new university observatory begun in 1899 is now completed. Near the observatory proper are two other buildings, one for the astronomers to live in, the other for a meteorological station. The principal dome contains a 12-inch equatorial. There are two transit-circles, one by Cooke and one by Bamberg.

Kiel. *H. Kreutz.*—"Centralstelle für astronomische Telegramme"; also responsible for the *Astronomische Nachrichten.* Computation of orbits of small planets.

Königsberg. *H. Struve.*—Dr. Cohn observed comparison-stars for Eros and Gill's zodiacal stars. In all Dr. Cohn made 5500 observations in R.A., and about 300 in Dec. The 13-inch refractor was employed in the early part of the year in observing Eros. The measuring of double stars was continued. Observations of Saturn were begun as a preliminary to a research on the system. The other observations are of Nova Persei, Encke's Comet, and newly-discovered minor planets.

Leipzig. *H. Bruns.*—With the heliometer Prof. Bruno Peter made series of measures for the parallax of σ Draconis and 61 Cygni. Dr. Hayn was employed on securing selenographical coordinates, the meridian work being done by Dr. Grossmann.

Milan. *G. Celoria.*—Owing to various causes little work was done with the 18-inch. Meteorological and Time service.

[To be continued.]

Bidston, Liverpool.—As the primary purpose of the observatory under Mr. W. E. Plummer's control is to satisfy the needs of navigation, perhaps anything of the nature of astronomical research should not be expected in his Report, although the Observatory contains an equatorial as well as a transit instrument. The equatorial is used for observation of such comets as they appear, and as these were few in number during the year 1901, the instrument was occasionally used for measuring double stars. The transit has been used in recent years for observing, besides the stars necessary for time-determination, certain circumpolar stars whose places were wanted by Dr. Auwers; but this work has been given up, because, as Mr. Plummer says, "an examination of the observations has disclosed sudden and uncontrollable fluctuations in the amount of the azimuthal error." As investigation of such fluctuations is always interesting, and often leads to valuable information, it seems hardly well advised to have abandoned these observations without having made some effort to discover the cause of the idiosyncrasy, but no doubt Mr. Plummer has made this effort. In the report of the seismological observations there are 111 entries of disturbances in the record, which, although the notes show that several are probably due to accidental causes, may be indications of small earthquakes. The number of instruments received for test and examination remains practically the same as in former years, but the number of sextants shows a slight increase. The daily meteorological observations and a summary for the year are included in the Report.

PUBLICATIONS.

Eclipse Meteorology and Allied Problems. (By Frank H. Bigelow, Professor of Meteorology.)—This work, which is published by the U.S. Weather Bureau, contains a full discussion of the observations made in North America in the eclipse of May 28, 1900. The shadow passed over or near a large number of permanent meteorological stations, and the results of all the stations within 500 miles of the central line are collected here; on the outer limit about $\frac{9}{10}$ of the area of the Sun's disc was obscured at mid-eclipse. Fine weather prevailed at most of the stations, and this makes it easier to obtain the effects of the eclipse uncomplicated by local sources of disturbance.

The barometer, which was rising at most stations, rose more rapidly at several of them about the time of totality; but this is not sufficiently pronounced to establish its connection with the eclipse.

The temperature effects are much more clearly defined. The fall began to be noticeable 45 minutes before totality. It reached a minimum some 15 minutes after totality, when it amounted to some 4° (Fahr.) on the central line, and was almost as great for 150 miles on each side, while 500 miles from the central line it was about 2°. After about two hours more the effects of the eclipse vanished and the normal diurnal curve was resumed.

The vapour-tension diminished 0·01 inch when the temperature was a minimum. This is only $\frac{1}{3}$ of the theoretical amount. The author suggests that the passage of the shadow was too rapid for equilibrium to be established, and in consequence "there was set in motion a rapid interchange of small currents . . . . . and this turbulent action along the edge of the umbral cone produces.... optical obscuration. The rays of light from the Sun struggling through these currents could reach the observer only in the form of fluttering waves......the well-known eclipse shadow-bands. The object seen in this way is the crescent of the disappearing or reappearing Sun."

The wind-velocity fell on the average one mile per hour (from six to five miles per hour) during totality. This he explains by the change of temperature, on the analogy of land and sea breezes. There was no distinct change in its direction. The author gives reasons against accepting the theory of an "eclipse cyclone" which has been advanced by Mr. Clayton and others.

The number of calories per kilogram of air absorbed by the shadow is next calculated. The maximum amount is 0·40 calories 30 minutes after totality. The absorption is sensible for 45 minutes on each side of the maximum.

The next chapter discusses the observations of the shadow-bands. These do not seem to have been very conspicuous at this eclipse. Their speed was about $6\frac{1}{2}$ feet per second. The bands were $1\frac{1}{3}$ inches wide, the spaces $2\frac{1}{5}$ inches. They were seen for about $\frac{1}{4}$ minute before and after totality.

Most observers gave the direction of motion as N.E., both before and after totality, but a few give it exactly opposite, so it evidently was somewhat puzzling to determine. The author's theory of the bands has been outlined above; the idea that they are connected with the fall in temperature is not new, but he works it out in a detailed and quantitative manner, and it certainly appears highly probable. We are surprised that he thinks it worth while even to allude to the theory that the bands are caused by diffraction at the Moon's limb; the observations at all eclipses make it clear that their velocity is enormously less than that of the Moon's shadow, which is absolutely fatal to the theory. It has, however, been advanced by many people who ought to know better.

The rest of the book is taken up with a discussion of the physical condition of the Sun and his surroundings. This is of interest both as giving a summary of previous speculations on the subject and as adding some very suggestive ideas of his own. Space does not permit of more than a brief mention of a few of his conclusions. He says of the polar plumes:—"The measures were discussed on the hypothesis that the polar curves are in effect magnetic lines of force seen in projection, and therefore distorted, except those springing from the visible edge of the disc.... These rays conform closely to the theory.....the bases of the polar rays are confined to a belt 10° wide, whose central line is 32° from the poles of the Sun. In other words, the polar regions are denuded of visible lines of force through an arc of 50°, 25° on each side of the pole, and then the rays spring up thickly in a sort of collar or ruffle, quite like the distribution of the terrestrial aurora..... It was easy to construct a small model showing the polar ruffle. The axis of the N. and S. polar crowns is not an exact diameter of the Sun, but the coronal poles are about 5° from the Sun's axis of rotation, and the S. precedes the N. by 100°." The author goes on to say that such a model enabled him to predict the details of the 1900 corona with remarkable success. The obvious want of symmetry between the east and west wings receives an explanation.

He deduces that the corona rotates in a period of 26·68 days, this period being deduced partly from a discussion of the features of different coronas, partly from magnetic variations. He supposes the equator and poles of the Sun to rotate in the same period, and gives the period of the middle of the spot zones as 27·3 days.

Both magnetic and electrical forces are invoked to explain the coronal manifestations of energy, and the distribution of these forces that he postulates in the Sun is shown to be in many respects analogous to what is known of the corresponding forces on the Earth.

Whether his conclusions be accepted or not, the work will repay perusal, as great pains have evidently been bestowed upon it, and a considerable mass of statistics of various kinds has been collected to throw light on the interesting cosmical problems under discussion. A. C. D. C.

ANNALES CÉLESTES DU DIX-SEPTIÈME SIÈCLE*. A. G. PINGRÉ.—In many ways this is a most interesting volume. M. Alexandre-Guy Pingré, well known from his 'Cométographie,' was born at Paris, 1711 September 11, and entered the *Congrégation des Génovéfains* at the age of 16. From the first he was much occupied with theology, and became mixed up in the quarrels of the "Jansénisme," and found himself kept in the various provincial colleges. When the surgeon Locat founded the Academy at Rouen, Pingré was already there. Having no astronomer, Pingré accepted the position, and at the age of 38 he devoted himself with so great ardour to astronomy that in 1753 he was nominated Correspondent of the Academy of Sciences (Paris). Shortly afterwards the "Congrégation" called him to Paris, where he fitted up a small observatory at St. Geneviève; at the same time he was appointed librarian of this celebrated abbey. From the first years of his return to Paris he formed the project of writing a history of astronomy in the 17th century, which should consist, in his opinion, of a collection of astronomical observations made from 1600 to 1700.

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* Published under the auspices of l'Académie des Sciences by M. G. Bigourdan. Paris, 1901, in 4° de xij+628 pp.

This project, approved by the Academy of Sciences in 1756, was not carried out immediately. For over 30 years from its conception Pingré did not lose any opportunity of collecting material. In the words of Lalande, he had "beaucoup de piéces détachés, que personne peut-être n'eut été en état de réunir."

When the work was finished, Pingré submitted it to the Academy. The report on it is very interesting, and we here give it in Lalande's words:—"By order of the Academy, M. Le Monnier and myself have examined M. Pingré's manuscript entitled 'Annales célestes du 17^{e} Siècle.' This work, which M. Pingré first announced in 1756, and of which the Academy approved, has been waited for with some impatience by astronomers. It contains a collection of every important observation made in the last century by Tycho Brahé, Kepler, Lansberge, Hortensius, Hévélius, Horoccius, Cassini, Picard, La Hire, Halley, Flamsteed, Auzout, Riccioli, Bouillau, Gassendi, Longomontan, Schickard, Hodierna, Agarrat, Féroncé, Vendelin, Mut, Kirch, Sedileau, Wurzelbau, Margraff, Eimmart, Zimmermann, Malvasia, Elie de Lewen, &c.

"The observations are extracted from a large number of works, brochures, journals, loose leaves, and various precious manuscripts, such as that of Bouillau, in the possession of M. Le Monnier, those from the Royal Observatory, communicated to M. Pingré by M. Cassini, the manuscripts of La Hire, Kirch, Sedileau, Margraff, and several others which Joseph de l'Isle has collected at the 'Dépôt de la Marine.' The volume contains exact calculations and results for the principal observations, also important errata in the various books from which they are extracted. For example, in the case of Tycho, in an introduction M. Pingré gives the observations omitted in the 'Historia Cœlestis Tychonica,' and the most essential corrections and errors which spoil this collection of Tycho Brahé's observations.

"The history of astronomy is here exposed by means of the phenomena given, the works from which they are extracted and the authors of those works, and the history has that kind of merit and interest which will render it valuable, equally with those of Weidler and Bailly.

"Some idea of the extent of this work can be gathered from the fact that the manuscript contains nearly 500 large folio pages; its importance may be estimated by the progress of astronomy in the last century, which produced new instruments, new discoveries, laws till then unknown, which saw everywhere new observatories, the establishment of academies giving a new impulse to astronomy. In this work M. Pingré collects all the facts of which astronomers have need for their researches, for their tables, for their calculations of planetary revolutions. In fact, it will form a work for constant reference, and one is astonished at the courage of M. Pingré in bringing to a successful issue a work so laborious and requiring for its execution all the sagacity and astonishing facility in calculation possessed by M. Pingré. We think that this most important work is well worthy of approval by the Academy and of being printed under its auspices."

Following upon this report the National Assembly granted 3000 livres for the publication of the work. This was in February 1791. The printing proceeded slowly, for at the death of Pingré, on the 1st of May, 1796, two thirds only had been completed, and it was then stopped. Indeed, not only the manuscript seems to have been lost, but the portion already printed was lost sight of. After searching for a long time in every place imaginable, M. Bigourdan found a complete printed copy of 364 pages, and the remaining portion in manuscript. The printed portion belonged to a Parisian bibliophile, who had acquired it as waste paper at a small village in the middle of France. It was Lalande's own copy, for the marginals are in his writing—amongst others the following:—"Lalande, 1796 . . . Barois m'a donné ces feuilles en novembre 1796. Il attend la paix pour finir." The remaining portion of manuscript was found at the Paris Observatory, also by M. Bigourdan. This MS. was catalogued under the false title of "Manuscrit Erasme Bartholin. Copie des observations de Tycho Brahé." The work being thus entirely reconstructed, the Academy decided to reprint *in extenso.*

We have thus the observations arranged year by year, from 1601 to 1700, in the following order:—

Eclipses of the Sun, and Solar observations in general.
Eclipses of the Moon, and general Lunar observations.
Observations of Planets.
Observations of Stars.
Miscellaneous Notices (Faits).

The first entry is the Solar Eclipse of 1601, Dec. 24, which was observed at Prague by Kepler with Tycho's instruments, and Pingré gives a detailed account because, as he remarks, he considered him the best observer. This observation was, of course, made without a telescope, in a dark room, with a clock marking minutes and seconds. Other observers of this eclipse were Longomontanus and Lansberge. Under the heading *Faits*, Pingré sums up the astronomical work of the year, indicating the death of celebrated astronomers, foundation of observatories, and the publication of important works. For example, on page 8, *i. e.*, the year 1601, is found the notice of the life and work of Tycho Brahé. In 1603 a critical note on the Uranometria of Bayer; in 1604 a note on Kepler's Optics; in 1609 Kepler's 'De motibus stellæ Martis' and the invention of the telescope; in 1610 Galileo's 'Nuncius Sidereus,' and discussion on the discovery of Jupiter's satellites; in 1611 the discovery of sun-spots; 1627 Rudolphine Tables.

We notice that our English astronomers of this period get full recognition. Thus we frequently see the names of Flamsteed, Crabtree, Halley, Horrox, Streete, Ward, and Newton. Thus, on page 329 is a notice of the appointment of Flamsteed as Astronomer Royal (1675); and on page 338 is an account of Halley and his voyage to St. Helena in 1676. Naturally many observations have lost the value assigned by Pingré; but such observations as those of the eclipses of Jupiter's Satellites, Occultations of Stars by the Moon have increased in value.

The contents, as we have already indicated, are arranged in chronological order, as Pingré himself planned; but we are indebted to M. Bigourdan for three Indexes, which enable us to get at any particular observation in a very easy way. These Indexes are arranged:—

(1) Authors in alphabetical order.
(2) Observatories and other places, also in alphabetical order.
(3) An alphabetical index of the whole subject matter.

It is thus seen that M. Bigourdan, besides discovering the lost work and looking after the printing, has materially added to its value, and too much cannot be said for his indomitable perseverance and devotion, when it is remembered that only a few months since he presented four volumes, 'Observations de Nébuleuses et d'Amas stellaires,' to the Royal Astronomical Society. As for ourselves, who have seen much of M. Bigourdan during the present "Longitude of Paris" observations, we heartily congratulate him. T. L.

THE PENNY CHRONOLOGY.—Mr. Lynn sends us a copy of the third edition of this little *brochure*, published by Sampson Low & Co., and brought up to the present time. The scheme is to give a selection of the dates of the most prominent events in the history of the world from the time when this could be done with accuracy; and a few of these relate to astronomical science.

NOTES.

Comet Notes.—A comet which promises to be of considerable interest was discovered by Mr. Perrine on the last day of August, and has been designated 1902 *b*. It was independently found by M. Borrelly at Marseilles on the following day, At the time of discovery it was of about the 8th or 9th magnitude, with a tail 8′ in length, and a stellar nucleus of about the 10th magnitude.

From its very favourable position the number of observations has been very large, and as it will be visible for many months the orbit should be determinable with great precision. The most accurate elements yet to hand are those of Prof. A. A. Nijland, of Utrecht, which are based on observations made on Sept. 1, 6, 10, and which accurately represent the observed positions of the comet up to the middle of September.

T	1902 Nov. 23·934 Berlin M.T.	
ω	132° 53′ 2″	1902·0.
☊	49 16 14	
i	156 16 42	
log q	9·60322	

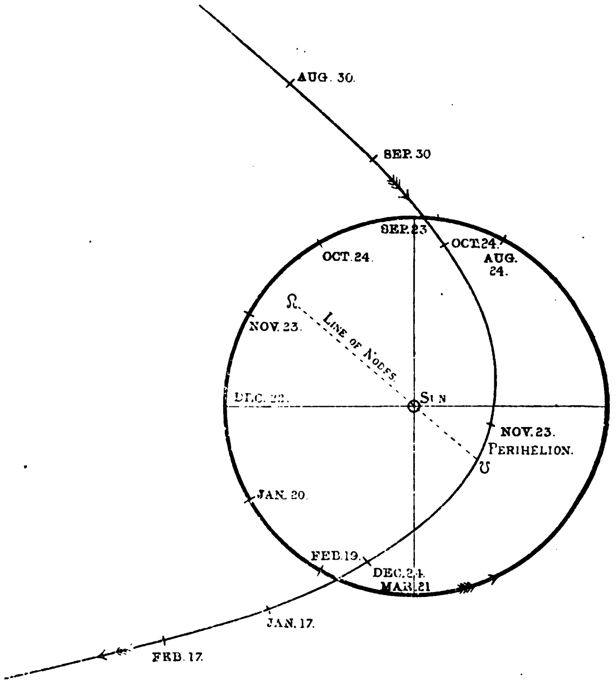
From these I have computed the following ephemeris for Greenwich midnight:—

Day.	R.A.			Dec.		Log r.	Log Δ.	Brightness.
	h	m	s	°	′			
Oct. 2	22	37	2	56	32 N.			
3	22	4	34	55	22			
4	21	32	44	53	35	0·0878	9·5794	25·2
5	21	3	4	51	15			
6	20	35	50	48	25			
7	20	11	35	45	12			
8	19	50	32	41	47	0·0612	9·5753	29·1
9	19	32	28	38	15			
10	19	16	50	34	41			
11	19	3	18	31	12			
12	18	51	24	27	51	0·0328	9·6213	26·8
13	18	40	56	24	43			
14	18	31	34	21	45			
15	18	23	17	18	59			
16	18	15	52	16	24	0·0012	9·6912	22·5
20	17	54	8	8	3	9·9676	9·7697	18·3
24	17	38	16	2	3 N.	9·9300	9·8375	15·9
28	17	26	20	2	25 S.	9·8890	9·9013	14·3
Nov. 1	17	16	12	5	53	9·8440	9·9559	13·7
5	17	6	36	8	42	9·7944	0·0047	13·7
9	16	57	0	11	5	9·7416	0·0463	14·5
13	16	46	46	13	12	9·6882	0·0815	15·7
29	15	58	52	20	38	9·6322	0·1452	15·2
Dec. 15	15	5	0	27	53	9·8340	0·0969	7·5
31	14	8	40	36	52	9·9950	9·9956	5·7
Jan. 16	11	55	20	46	7	0·1076	9·8863	5·6
Feb. 1	8	38	40	38	23	0·1926	9·8885	3·7
17	7	3	36	20	56 S.	0·2600	0·0254	1·5

It will be seen that the comet is nearest the Sun (distance 0·40) on Nov. 23, nearest the Earth (distance 0·37) on Oct. 6, brightest on Oct. 8. It again approaches the Earth to within distance 0·76 about Feb. 8. It is, however, quite probable that the actual brightness will go on increasing throughout October, since the formula $\frac{1}{r^2\Delta^2}$, used in computing it, takes no account of physical increase of brightness, which is extremely probable in the case of a comet approaching pretty near to the Sun.

A photograph taken at Greenwich on Sept. 26 shows a fan-shaped tail with at least four rays, the longest being traceable for fully 1°.

Two diagrams are appended, one showing the orbits of the Earth and Comet, the other the path of the Comet through Cygnus and Ophiuchus. (Frontispiece.)



After perihelion passage it will be best seen in the Southern Hemisphere, but about February next it may possibly be still telescopically visible in England.

Another comet (1902 *c*) was discovered last July by Mr. John Grigg, of Thames, New Zealand, and observed by him on several evenings. Unfortunately his positions are too rough to deduce a reliable orbit, so, unless the object should be discovered on the Harvard photographs, it must be regarded as lost. It may be worth while to point out that amateurs in similar cases can determine the comet's place with considerable accuracy by making careful drawings of the small stars in the field, and alligning the comet from these. If the observer has not star-catalogues at hand he can send the diagrams, or tracings from them, to any well-equipped observatory, when the position of the comet can be deduced within a minute of arc, provided reasonable care is taken in the drawing. Had Mr. Grigg done this there would have been material for obtaining a close approximation to the orbit.

A. C. D. C.

Minor Planet Notes.—The planet JJ announced two months ago proves not to be new, but identical with (451) Patientia.

The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Mag.
JL	July 9	Carnera.	12
JM	Sept. 2	"	Not stated.
JN	" 4	Wolf.	12
JO	" 3	"	13
JP	" 3	"	12
JQ	" 3	"	12
JR	" 3	"	12
JS	" 7	"	13

The identity of HS with planet (359) is confirmed beyond doubt. This planet has been named Georgia.

Ast. Nach. 3805 contains a note by M. Ebell on planet GT. A plate taken at Heidelberg, 1901 Dec. 3, showed two planets, of which one was thought to be new and was named HM, while the other was thought to be GT. Reasons are now given for supposing the first one to be GT, and that this planet GT is identical with (156) Xanthippe.

The number of planets announced as new that subsequently turn out to be already known may be taken as evidence that the exhaustion of the group, though by no means reached, is approaching. The Heidelberg plates appear to show about four known planets for each unknown one, so that at the present rate of work we may expect that in another ten years very few planets as bright as the 12th magnitude will remain to be discovered. To discover, however, is not always to keep, for there is a long list of planets that have been seen at one opposition only and then lost again.

The planet Eros is now a morning star, of about the 12th magnitude, and has recently been photographed at Greenwich.

A. C. D. C.

The Mirror of the Crossley Reflector.—As it seemed from a remark in our last number that there is still some doubt as to the maker of the 3-foot reflector now at Lick, it has been suggested to us that it may be well to publish the following letter written to the *Astrophysical Journal*:—

"Professor Keeler's most useful article describing the Crossley Reflector of the Lick Observatory, in the *Astrophysical Journal* of June, 1900, contains the following paragraph:—

> "The large mirror, the most important part of the telescope, has an aperture of 3 feet, and a focal length of 17 feet 6·1 inches. It was made by Mr. Calver. Its figure is excellent. On cutting off the cone of rays from a star with a knife-edge at focus, according to the method of Foucault, the illumination of the mirror is very uniform. While the star-discs as seen in an ordinary eyepiece are small and almost perfectly round, they are not, I think, quite so good as the images seen with a large refractor; still, they are very good indeed, as the following observations of double stars made recently for this purpose will show.

"A correspondence with Sir Howard Grubb and Mr. J. Gledhill has proven to me that the figuring of this excellent mirror is the work of Sir Howard Grubb. Mr. Crossley's gift to the Lick Observatory included two mirrors essentially of the same diameter and focal length. These were distinguished by the letters A and B. An extract from Mr. Gledhill's letter is as follows:—

> "When we found that the A mirror was not good, and that the figure of the B was a little worse, we asked Dr. Common and Mr. Wassell (an expert in mirror-testing by the Foucault method) to come and test them. They did so. Sir Howard Grubb was then called in, and he examined both mirrors. All the results of the tests agreed with ours. It was then decided to send the B mirror to be refigured by Sir Howard Grubb. This was done, and you have it as it came from his workshop.

"It is the B mirror which has been used in all the work with the Crossley Reflector at the Lick Observatory.

"In justice to Sir Howard Grubb I herewith make the correction which of course the late Professor Keeler would very gladly have made. "W. W. Campbell."

The second series of observations for the re-determination of the Paris—Greenwich Longitude is now in progress. The first part of the series, in which M. Lancelin and Mr. Hollis were the observers at Paris, is completed; and these two members of the party are now at Greenwich, M. Bigourdan and Mr. Dyson being at Paris.

THE U.S. NAVAL OBSERVATORY.—Captain Colby M. Chester, U.S.N., has been appointed Superintendent of the Washington Observatory, in place of Capt. C. H. Davis, who has been detailed for duty at sea. A note in *Popular Astronomy* says:—"From recent Washington and New York papers it is evident that things are being shaken up to the very bottom. Some excellent men, who have heretofore enjoyed an enviable reputation as high-minded, intelligent, and scholarly men, have become mixed up in this Naval Observatory quarrel in such a way as to besmirch their good names, with (probably) undeserved and even shameless ridicule."

THERE will be a Total Eclipse of the Moon just before sunrise on the morning of October 17. The circumstances will be found in the *Companion*. There will also be a Partial Eclipse of the Sun, partly visible at Greenwich, on October 30.

IT is reported that the meteorological work at Ben Nevis Observatory is to be continued, and it is believed that the Treasury are likely to put this institution on a sound footing financially.

THE obituary list for the past month includes the name of Dr. H. von Wild, who established the Russian Meteorological Service, and was Director of the same from 1868 to 1895. The Meteorological and Magnetic Observatory at Pawlowsk, where excellent work has been done, was founded under his direction. Dr. Wild was in his 69th year at the time of his death at Zurich.

WE learn from *Popular Astronomy* that Prof. W. S. Burnham, of the Yerkes Observatory, has given up, for the second time, a lucrative business, that he may give his whole time and energy to practical work in astronomy.

THE attention of foreign and colonial readers is called to the third of the notes in the Oxford Note-Book (p. 385). We will gladly publish opinions on astronomical topics in the form of short notes if anyone likes to communicate such; but, as is pointed out in the note, there are dangers, and we must reserve to ourselves the usual editorial privilege.

FROM AN OXFORD NOTE-BOOK.

THE Belfast Meeting of the British Association was thoroughly enjoyable and successful, and the Subsection of "Astronomy and Cosmical Physics" shared in the general success. The experience of the last two years showed that Astronomy alone scarcely provided material for a separate department; astronomers do not come in sufficient numbers to the meeting, and their papers are, for excellent reasons, usually published elsewhere. But by associating with astronomy the other "observational" sciences of meteorology, magnetism, and seismology, a very strong subsection was formed—rather too strong, indeed, for the audiences showed a tendency to desert the longer-established subdivisions of Section A for the new one. Prof. Schuster, in his opening address, took the opportunity of saying some earnest and courageous words about the observational sciences in general, pointing out the special dangers to which they were liable, and urging that a drastic remedy should be applied, at least in the domain of meteorology: viz., that *if* it be impossible to analyse and digest the already accumulated results, on lines which he indicated, without stopping the observations, *then* the observations should be stopped, and the problems waiting for solution attacked with the energy thus set free. His words are reproduced elsewhere in this number; and here it need only be remarked that similar conclusions have already been reached by some astronomers (*e.g.* where current transit-circle observations have been curtailed, or stopped altogether, in order to bring up arrears), and that the spirit of his remarks with reference to meteorology is at least as old as De Morgan, as readers of the 'Budget of Paradoxes' will recognize.

THERE were so many papers that three full morning sittings were occupied with them. The exhibitions of lantern-slides by Dr. I. Roberts, and of a set made at the Yerkes Observatory, were naturally much appreciated by a British Association audience, and none of the papers were too technical to excite general interest. The addition of meteorology and seismology to the Section had happy results, as we heard from Mr. W. N. Shaw about the kite-flying experiments made so ably by Mr. Dines, and from Prof. Milne about the present state of seismology, with special reference to the recent volcanic disturbances. There was evidence that Mr. Hinks, as Secretary to the Subsection, had worked most energetically, and much of its success was undoubtedly due to him.

Some three years ago, in the *Publ. Astr. Soc. of the Pacific*, there were started some paragraphs headed "General Notes," to which I offer a tardy but respectful word of welcome. We scarcely get enough comment and criticism in our astronomical journals, *i. e.* comment of a light and general kind. Controversial points raised in one memoir are of course suitably and seriously replied to in another; but this is not the kind of criticism I have in mind. We should like to know the views of American astronomers (and of course others at a distance) much as we should if we paid them a visit. Those who have been fortunate enough to pay such visits know what a vast amount there is still to be learnt from a man whose strictly scientific writings they may already know thoroughly well. Of seeing material observatories one perhaps gets a little tired: "I sometimes dread visiting a new observatory," said an enthusiastic astronomer to me the other day, "lest they should insist on showing me every instrument in the place." But it is otherwise with the *inhabitants* of the observatories; there must be something very deficient in the visitor if he does not profit by the opportunity of talking to workers in the midst of their work. No doubt, therefore, those who can travel will always learn most of their fellow-astronomers: but for those who must stay at home cannot a little more be done? Cannot they be *talked to* in print a little more? There are of course dangers ahead: the airing of views on all topics can only too easily be made tiresome or even objectionable; but as a personal opinion I would hazard the view that at present we could afford to run a few risks, if haply we may learn a little more about one another.

The particular paragraph which gave rise to the above reflections is that on p. 147 of vol. xiv. *Pub. Ast. Soc. Pacific*, where the writer boldly says a word for the "popular article." I should like to express sympathy with him, though at present the main point is not so much how far we agree with him as that we should have the pleasure and benefit of seeing his views, whatever they may be, expressed. I will venture to reproduce them here:—

> It has been urged more than once against the *Jahresbericht* (Wislicenus) that too much space is devoted to reviewing popular articles. In this criticism we cannot concur; the work is thereby made more useful to amateurs, and it must be confessed that professional astronomers will profit by it as well,—for it is notorious that the latter, as a rule, do not spend much time upon technical papers outside of their own specialities. If they get a broad knowledge of their science at all, it is quite usually through the medium of popular exposition. In this connection we may recall the frank statement of the late Professor P. G. Tait: "I have abstained from reading the details of any investigation (be its author who it may) which seemed to me to be unnecessarily complex. Such a course has inevitably certain disadvantages, but its manifest advantages far outweigh them."

It would be well if we could all learn the lesson conveyed in the above words: *i. e.*, we may arrive at our result how we like, but in expounding it in print we must cast it into simply readable form. Prof. Tait himself used quaternions freely to get his results, but of course he printed them in the more popular notation. It was a familiar sight to see him in the club-room at St. Andrews smoking a pipe and revising for press* one of his elaborate investigations on a half-sheet of note-paper — a conciseness rendered possible by the quaternion notation, which it must have cost him something to discard in print. Alas! we shall no more see there either himself or the soldier-son of whom he was so proud, and of whom it was usually said that "when his father had shown mathematically that a golf-ball could not be driven further than a certain distance, 'Freddy' promptly drove one further." Put succinctly this was the actual fact; theory and observation were in disagreement, and, as on so many previous occasions, theory had to find out its mistake. Prof. Tait had in fact, in his early calculations on the flight of a golf-ball, neglected the effects of "underspin"; and without underspin it is practically impossible for human beings to drive a ball 200 yards (to first pitch). When this limit was far exceeded ("Freddy" drove over 300 yards carry, 390 yards "with a run") he recognized that there must be some defect in the theory, and ultimately found it in the "underspin," which instead of being negligible as he supposed, was the most important factor in good "driving." How he gradually but completely established this point concerns physicists and golfers rather than astronomers: but the battle between theory and observation, and its inevitable result, is of interest to us all—especially perhaps to Mr. S. C. Chandler, who enjoyed the triumph of observation so recently. To save possible misunderstanding, I should add that the historic drive above mentioned was (if my memory of dates is correct) not the actual evidence which led Prof. Tait to correct his theory: it came later: but it is none the less a striking instance of the possible error of a theoretical calculation deliberately made by a scientific man in the front rank. The distances I have mentioned are authentic. Almost the last time I saw Prof. Tait he walked with me to the spot and told me all the facts himself. In case this should meet the eye of anyone seriously interested I will briefly give them:— The drive was from the tee at the 13th hole at St. Andrews (right-hand course) which was set back about 50 yards at the time, being *behind* the green of the previous hole. The ball was seen to pitch on the plateau by the players in front over whose heads it went. They were two brothers of "Freddy" and had derisively invited him to drive at them; and one of them (a surveyor) was so amazed at the result that he walked back into the town and brought out his theodolite to survey the distances: finding as above that the "carry" was over 300 yards, and the whole drive 390 yards. The ball was found in the bunker near the hole.

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* His collected works were being published by the Cambridge University Press.

ALL this has carried me, I fear, rather far away from astronomy. But there seems to be some mysterious connection between astronomy and a golf-links; for the following astronomical observations "on the links" are reported in *Punch* for Aug. 27:—

> INCREASED LATITUDE ON THE PART OF THE MIDNIGHT SUN:—"On the Links the Signallers of the Dundee City Rifles spent the hours of darkness manipulating the heliograph."

AN announcement made by the Director of the Lick Observatory, in Bulletin No. 20, is one to be hailed with delight. Copies of the photographs taken at the Lick Observatory are to be generally obtainable at a moderate cost, it being "assumed that such copies would be used for purposes of study and investigation." I cannot but think that this is the right method of dealing with the rapidly accumulating stores of photographs at our large observatories. Copies cannot be indefinitely presented to those who can profitably use them, however generous may be the wishes of the Directors: and the few shillings per copy which would put the scheme of supplying them freely on a working basis will not be grudged by anyone. The price should be fixed high enough to make things work quite smoothly—it would be far better to make a profit than to run a risk of loss: for if necessary the profit can easily be turned to the advantage of astronomy, or even of the purchasers themselves, in some way. The Royal Astronomical Society started a scheme of this kind a few years ago which has worked quite well and has shown *inter alia* that there is a steady demand for astronomical photographs—chiefly in the lantern-slide form at present. A list of those hitherto published is given year by year in the February number of the *Monthly Notices*, or can be had separately on application to Mr. Wesley. To avoid commercial difficulties (which are also hinted at by the Director of the Lick Observatory) the R.A.S. photographs are only supplied to Fellows, but those who are not Fellows can generally find some friend to make the purchase for them. I wonder whether Professor Campbell will be able to revise his opinion that "the collection of photographs in our possession is so extensive that it is not practicable to supply a detailed list"? I sincerely trust the decision may not be permanent, for half the value of the collection will in that case be lost. It is most important for a worker to know what photographs are available of the region he is at work upon; and I would urge, as a tentative scheme, that *beginning from the present time* (and leaving arrears for future consideration) the Lick Observatory should publish in its Bulletins a list of the plates taken, with the following particulars:—(1) Reference No. of plate. (2) Date of exposure and hour-angle; or simply date to five decimal places, which identifies the hour-angle. (3) R.A. and Decl. of centre for 1900·0. (4) Approximate angular radius of field in minutes of arc. (5) Approximate magnitude of faintest star photographed. Other details might of course be useful, but these five would generally enable a worker to know whether the plate would be likely to be of value to him, and he could order it by its number. The work of cataloguing on this plan the existing plates might proceed gradually—say a year at a time, beginning with the early plates, which are liable to be most important in investigations of proper motion or other change. Would such a plan be found generally useful? I should welcome the opinion of other astronomers in this matter.

AMONG attempts to express angular measure by means of lengths, the following description of a meteor takes high rank. It is from the *Times* of August 23, 1902:—

> A FIERY PHENOMENON.—Mr. C. F. Dowsett writes from Winklebury, Basingstoke, under yesterday's date:—"A few minutes after 2 of the clock this morning, in brilliant moonlight, I saw a ball of fire passing in a straight line from west to east. If an ordinary star appears to our vision about the size of half-a-crown this ball of fire appeared to be fully as large as half the moon appears to us. I was at a window 15 ft. from the ground level, which is about 400 ft. above sea level. In passing the ball of fire appeared just below the tops of trees which are 63 yards from the window and about 50 ft. high, and when past them I watched it in the open (as far as the window would allow me) over about two miles of the earth's surface. Just as I was losing sight of it portions seemed to descend from it and a part to go on. Judging from earthly motion, it seemed as if travelling at about the rate a train does when making 40 miles an hour. Perhaps some of your readers had a more extended view."

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A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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The System of η Geminorum.

The inclusion of η Geminorum among the stars discovered at the Lick Observatory to have variable radial velocities claims, for several reasons, particular attention. To begin with, it is visually double. Close to the glaring red disc of the variable, Mr. Burnham detected, in 1881, a tenth-magnitude companion, which proves to be in slow retrograde revolution. The latest published measures —executed more than five years ago—showed a decrease of the angle by 10°, accompanied by a slight augmentation of the distance, which now amounts to just 1″. Centuries, indeed, may elapse before it becomes possible to compute the orbit of this slowly-moving pair; yet even a remote prospect is better than none at all; and η Geminorum is the only star of its spectral class that has so far given the smallest opening for enquiries into its gravitational regimen. And the opening will be rendered comparatively wide should the intimation of its forming a ternary combination be verified by the progress of research.

Several other binary stars have already been resolved spectroscopically into three components. Castor is an illustrious example; and it is followed by κ Pegasi, by ξ Ursæ Majoris, and perhaps by ζ Herculis. But the circumstances of η Geminorum are peculiar. It is the first third-type star found to be spectroscopically double. A beautiful range of flutings corresponds, in its analyzed light, to the fiery tinge of its colour. Moreover, it is essentially, though not very markedly, a long-period variable. That is to say, a cycle of 231 days is pretty regularly conformed to, while the changes comprised in it are much more feebly accentuated than in the generality of its congeners. And it is worth noting that the decline, not the recovery, of lustre is the phase sometimes imperfectly accomplished. At maximum of 3·2 magnitude, the object never sinks below 4·2, and is liable to halt in its descent at 3·7 magnitude. Thus, in the year 1879, the minima due in

February and November were alike observed by Schmidt as only half-developed or abortive*. Nor are the maxima attended by any symptoms of emission. No vivid rays of blazing hydrogen relieve absorption-bands resembling those of Betelgeux rather than the dense shadings of Mira. Nevertheless, the variability of this star, albeit moderate in amount, has the fundamental characteristic wanting in the equivalent fluctuations of many analogous objects. It obeys a definite period. It is no casual flickering. Some condition seems to be added to those prevailing in the constitution of irregular variables, by which a certain order of recurrence in time is enforced. And we cannot but ask ourselves the question, What may that condition be?

A possible answer is suggested by Professor Campbell's recent announcement that η Geminorum is a spectroscopic binary †. For, should the period of revolution turn out to be identical with the period of variation, a strong presumption would arise that companionship in some way dictates the course of light-change. That such a relation exists in the case of "Cepheid" variables has been fully established; but their rapid and precise fluctuations are of a very different character from the loosely regulated vicissitudes of stars like Mira Ceti. Nor has a scrap of evidence favouring the supposition that such variables are in reality crypto-doubles been until now produced. The assignment of this character, then, to a star periodical in nearly eight months is of somewhat critical import.

The first point to be ascertained is whether its two kinds of change synchronize—whether its light-cycle corresponds with its orbital cycle. So far, there is nothing to show that it does. The published observations are too few to supply grounds for a decision, or even for an opinion, on the subject. They extend from January 15, 1900, to February 2, 1902, during which time six plates were taken and measured by Messrs. Reese and Stebbins. The indicated range of velocity is small, only ten kilometres a second; but it is unlikely that the maximum rates were hit off at the epochs of exposure. It happened, however, that two determinations were made within six days of each other—on January 15 and 21, 1900—and they gave nearly the same result; the orbital speed had altered very slightly in the interim. Hence the inference must be drawn that the period of revolution is either comprised within a few days or is protracted over many months. The latter appears the more probable alternative: yet the further measures given lend only a shadowy countenance to the hypothesis of cyclical coincidence in light and motion-change. Observations made night after night for the express purpose of settling the matter once for all are, then, urgently needed. This is, indeed, a test-case as regards long-period variability. Even if all stars of the Mira class prove to have close companions, the spectral displacements occasioned by them might be very difficult of detection. But they will be keenly looked for should an agreement of periods make it clear that the binary condition of η Geminorum is no accident, but an indispensable part of its economy.

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* *Astr. Nach.* No. 2297.

† 'Bulletin of the Lick Observatory,' No. 20.

Acquaintance with the orbital relations of the star will, moreover, enable us to gather some preliminary ideas regarding its mass. A minimum estimate of this important element can, at any rate, be arrived at, when we know in what period and with what velocity it revolves. And since we are at present totally ignorant as to the amount of matter contained in third-type stars, and their probable mean density, the information in prospect is more than commonly desirable. Theories of their development, for instance, would be materially affected by it. Their place in the stellar hierarchy cannot, with any confidence, be assigned until sure data are provided respecting their size and consistence. Some among them are undoubtedly gigantic orbs. All seem to be remote from the Earth, beyond the measure of remoteness of solar or even of Sirian stars; and many shine with great intensity, notwithstanding powerful self-absorption. The conjecture might indeed be hazarded that great mass is a condition *sine quâ non* for the display of a fluted spectrum—that only bodies on a vast scale of construction survive to reach the Antarian stage, while minor globes sink towards extinction by insensible gradations, without undergoing any striking transformation in light-quality.

A proper motion of 0″·069, assigned by Dr. Auwers to η Geminorum, was pronounced by Mr. Burnham to be "obviously common to both (visible) components." From its amount we can derive an approximate notion of the probable distance of the system. Annual parallax, taken all round, naturally stands in a certain relation to proper motion. Let us put the ratio at one to seven and see what it comes to in the example under consideration, fully recognizing, at the same time, that an individual result of the kind is worthless except for illustrative purposes. Assuming, then, the parallax to be one-seventh of its proper motion, we obtain for it a value of just 0″·01, corresponding to a light-journey of 326 years. Our own Sun, relegated to such a distance, would probably have escaped enrolment in the Bonn Durchmusterung; it would take rank among the submerged and unnamed millions of space. In other words, its apparent brightness would be nearly seven magnitudes below that of η Geminorum. It would follow that the star actually emits five hundred and twenty times the light of the Sun. And even though this estimate be very far from exact, still the probability of some such enormous disparity subsists; and we are fully entitled to look upon the system of η Geminorum as no less interesting by its complexities than imposing by its scale.

Agnes M. Clerke.

Remarkable Meteors.

Certain large meteors, owing to circumstances favouring their apparitions, are witnessed by a great number of persons and attract considerable notice in the newspapers and scientific journals. Other meteors, equally brilliant and sometimes even more interesting and noteworthy, scarcely receive any attention whatever! Of very fugitive character, or appearing in the small hours of the morning or in a sky overcast at many stations, they pass comparatively unobserved. The fine, flashing fireball of Sunday night, July 13, had numerous spectators, for, apart from the convenient time of its display, it left a durable streak which enabled people, on looking upwards, to realize at once the nature of the phenomenon. And the fireball of August 22, though it appeared at 2 in the morning, was seen by many, for its long duration of about 40 seconds, even more than its glaring aspect, led to its being observed by everyone who happened to be out of doors at the time. But the conditions were different in respect to the fireball of the early evening of September 25, $7^h\ 35^m$, which gave a very vivid, lightning-like flash, illuminating the E. coast of Ireland, St. George's Channel, Cardigan Bay, and the coast of Pembroke, Cardigan, and Carnarvon with an astonishingly sudden and transient brilliancy. The meteor's descent was unusually rapid and short, so that people who were startled by the flash ascribed it to lightning, and thought no more of the incident. At Bristol I was observing Jupiter in a rather foggy sky when a sudden flash caused me to look quickly to the west, and I saw a faint green streak which disappeared in the fraction of a second. I had glimpsed it well enough, however, to fix its position from $211^\circ\ +41^\circ$ to $204^\circ\ +38\frac{1}{2}^\circ$, and note the direction from a point a little N. of Vega. At Enniscorthy, Ireland, the Rev. W. F. A. Ellison was looking towards the square of Pegasus, when everything was suddenly lit up and a nearly vertical, green streak, of momentary duration, was seen projected across β Pegasi, and extending from $340^\circ\ +31^\circ$ to $356^\circ\ +17^\circ$. Mr. Ellison remarks that the fireball descended so swiftly that the eye could not follow it, and but for the streak he might have concluded the flash to have been ordinary lightning. The duration of flight could not have exceeded one-eighth of a second. The real path of the object was over the sea between St. George's Channel and Cardigan Bay. It fell from 88 to 57 miles along a course of 34 miles, and if Mr. Ellison's value for the observed duration is correct, the velocity must have exceeded 250 miles per second! Quite possibly the duration may have been underrated, but extending it most liberally will not negative the fact that the motion of the meteor must have been greatly in excess of that consistent with parabolic velocity. The actual flight of the object was unfortunately not observed at Bristol, but the very sharp flash and evanescent streak sufficiently indicated that the speed was quite abnormal. The radiant was at 255° +46°, and from or near this point in the northern region of Hercules several large, flashing meteors were directed during the last week of September this year. On September 24, 11^h 53^m, Mr. C. L. Brook, of Meltham, near Huddersfield, recorded a small fireball falling in two seconds from 25½° −14½° to 30½° −21°. On September 26, about 9 P.M., a meteor brighter than Jupiter was seen at Alloa, Scotland, shooting from the region of α Cygni to a point between α and β Andromedæ, and leaving a short streak. On September 27, at 7^h 33^m, a fireball much exceeding Venus was seen by the Rev. W. F. A. Ellison at Enniscorthy, and by Mr. A. Sullivan at Dublin, in the region south of Aquila, and directed from the N. border of Hercules. On September 29, 10^h 16^m, a brilliant meteor was observed by Messrs. A. McDougall and W. E. Rolston at Earlsfield, Surrey, descending slowly from S.S.W. to S.E. These, and several other objects seen at about the same period, may not have belonged to one and the same system, but the general accordance in their directions is significant. On September 29 the shower of Herculids certainly continued active, for during a watch of three hours I recorded four meteors directed from 255° +46°. The motions, however, varied considerably, and I append the apparent paths of three of the objects which were very well observed:—

Date.	G.M.T.		Mag.	Path.				Notes.
				From		To		
1902.	h	m		α °	δ °	α °	δ °	
Sept. 29......	10	53	2	51	+15	54¼	+ 5	V. swift; white.
,, ...	11	26	2	5½	+42½	13½	+35	V. swift; white.
,, ...	12	52½	6	99	+78	85	+57	V. v. slow; yell.

Duplicate observations of these would be valuable, as it seems difficult to admit that meteors offering such great contrasts in their velocity can belong to a common radiant.

As a meteoric phenomenon of special character and differing widely from the very swift-flashing meteor of September 25, I may again mention the fireball of August 22, 2^h 2^m A.M. I computed the length of its observed path as 611 miles, which is certainly a record for meteors seen in England during the last 20 years. It fell from about 65 to 33 miles, and traversed the English Channel with a slow, horizontal course from W.S.W. to E.N.E., at a velocity of about 15 miles per second. It proceeded from a radiant just S. of λ Antinoi, near the W.S.W. horizon, and the duration of its luminous course was about 40 seconds. The estimates differed greatly, as they invariably do in such cases, and it may be interesting to quote from the various descriptions :—

Phenomenal slowness.
Very slow.
30 seconds.
At a terrific rate.
=train going 40 miles an hour.
At a great pace.
Excessively slow.
40 seconds.
Darted across sky.
Three minutes.
30 seconds.

40 to 50 seconds.
Of short duration.
At a great pace.
30 seconds.
Passed quickly across sky.
Two minutes.
30 to 35 seconds.
Visible for a long time.
10 to 12 seconds.
Comparatively slow.
Moved very steadily.

I corresponded with many of the observers, and elicited more definite information than that given in their original accounts. In some cases this proved useful. In one instance an observer was asked if he could give an estimate of the altitude and apparent diameter of the object. He promptly replied that "the fireball was 500 yards in height, and appeared to be 25 inches in circumference"! Three Australian troopers who saw the meteor from Hyde Park Corner thought the "tail 6 inches long"; a Plymouth seafaring man estimated it "2 feet 6 inches to 3 feet long"; while a Polperro fisherman considered the whole length to be several "hundreds of feet."

As an instance of velocity slower than theory would indicate, I may refer to a pretty bright 2nd-mag. meteor recorded by Mr. A. King at Leicester on July 28, $11^h\ 7\frac{1}{2}^m$, from $288° +1\frac{1}{2}°$ to $274° -3°$, and by myself at Bristol at same time, from $358° +35°$ to $330° +33°$. The estimates of duration were 2 sec. and 1·8 sec. The real path extended over 43 miles, so the resulting velocity was about 23 miles per second, which is little more than half parabolic speed. The radiant-point was at about $44° +21°$, near N.E. by E. horizon, and the meteor passed from nearly over Reading to Amesbury at heights from 74 to 71 miles.

The motion of several meteors doubly observed during the past two years has offered some striking and suggestive discordances, and it begins to be evident that the velocity of a meteor cannot be correctly ascribed from the mere position of its radiant. It is very unfortunate, however, that ordinary estimates of the flight-durations are liable to rather large errors, and it is to be hoped that photography will ultimately enable more accurate determinations to be made.

Bishopston, Bristol,
1902, Oct. 6.

W. F. Denning.

The Total Eclipse of the Moon, October 16.

It was not anticipated that this eclipse would repay observers for the irksomeness of rising at 4 A.M., as the altitude of the Moon was small and the total phase did not occur till twilight had made considerable progress. The weather conditions were, however, perfect, and the air was of a clearness and purity quite unusual in the neighbourhood of London; hence those who took the trouble to rise were rewarded by a spectacle of singular beauty and interest.

The watch was begun at $16^h\ 5^m$, at which time the smoky penumbra was already obvious on the Moon's eastern limb. I may remark that in other eclipses I have been able to trace the effect of the penumbral shading for about half an hour before or after contact with the shadow.

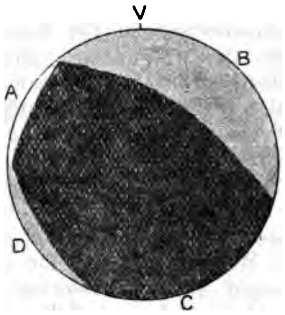
First contact was noted at $16^h\ 17^m\ 15^s$ G.M.T., or 21^s later than the 'Nautical Almanac' time. The difference may be partly due to the augmentation of $\frac{1}{50}$ which the 'Nautical Almanac' applies to the geometrical shadow; this augmentation is probably too great, implying as it does that the atmosphere at the height of 80 miles is sufficiently dense to obstruct nearly all the Sun's light.

Only two crater phenomena were noted, viz.:—

Aristarchus wholly immersed $16^h\ 36^m\ 20^s$ G.M.T.

Copernicus E. limb on edge of shadow $16^h\ 37^m\ 37^s$ G.M.T.

Other eclipses have shown such large discordances between different observers for these phenomena that it appears useless to expend much time upon them.



Sketch of the Moon's aspect shortly before Totality.

The eclipsed limb was not quite invisible even during the early stages of the eclipse; it was more readily seen as the bright crescent diminished.

At $16^h\ 56^m$ I noted that the upper portion of the shadow in the inverted image was very much brighter than the rest, an arrangement which persisted as long as the Moon could be followed.

At 17^h 15^m the appearance was somewhat as shown in the sketch (p. 395). V is the vertex of the inverted image, A the disappearing sunlit crescent; the portion marked B was of a bright copper-colour, that marked D a bright steel-blue, while C was of dark slaty grey and showed little trace of colour. The time I noted for the disappearance of the last ray of white sunlight on the disc was 17^h 18^m 42^s G.M.T., which is 6^s later than the corresponding time in the 'Nautical Almanac.' The boundary of the shadow was not, however, sufficiently definite to note the time with certainty within several seconds.

The totally eclipsed Moon now presented a dull murky orange appearance in the rapidly brightening sky. The superior brightness of region B persisted, but region D grew fainter and less blue as it became more deeply immersed in the shadow. The Moon was followed in the telescope till 17^h 40^m and with the naked eye till 17^h 43^m; its altitude was then only 7° and it was sinking into the smoky pall that nearly always hangs over London. Sirius, Procyon, Rigel, Betelgeux, Aldebaran, Capella were still staringly conspicuous objects, but their altitudes were of course much greater than that of the Moon.

The dawn on this day was of a bluish-white colour with no admixture of red or other warm colours. I mention this as it may possibly throw some light on the bluish patch on the Moon described above. A. C. D. C.

A Revision of Wolf's Sun-spot Relative-Numbers.

Prof. A. Wolfer, the successor of Dr. Rudolf Wolf, of Zurich, who has carried on the well-known tabulation of sun-spot statistics since the death of his predecessor in 1893, has lately published a new edition of the sun-spot relative-numbers, in which some inaccuracies of the earlier tables have been corrected, and which, besides being brought up to date (1901), comprise many observations found and worked up since the publication of the last edition. The following table, published by permission of Prof. Wolfer, is taken from the number of the *Monthly Weather Review* (Washington Weather Bureau) for April last. In that publication the sun-spot numbers observed or interpolated for every month between January 1749 and December 1901 are given, and the mean of these for each year taken. These yearly means are given in the second column of the table below. Further, in the above-named publication the monthly numbers have been smoothed by taking the means of every consecutive twelve, and these smoothed numbers for each month given. The annual means of these are taken and are given in the third column of Table I. which follows.

TABLE I.

Year.	Observed number.	Smoothed number.	Year.	Observed number.	Smoothed number.	Year.	Observed number.	Smoothed number.
1749 ...	80·9	—	1800 ...	14·5	15·0	1851 ...	64·5	63·2
1750 ...	83·4	83·1	1801 ...	34·0	33·7	1852 ...	54·2	52·8
1751 ...	47·7	52·2	1802 ...	45·0	44·1	1853 ...	39·0	38·6
1752 ...	47·8	45·9	1803 ...	43·1	43·0	1854 ...	20·6	21·0
1753 ...	30·7	28·9	1804 ...	47·5	46·8	1855 ...	6·7	7·7
1754 ...	12·2	13·5	1805 ...	42·2	42·5	1856 ...	4·3	5·2
1755 ...	9·6	9·3	1806 ...	28·1	27·3	1857 ...	22·8	23·0
1756 ...	10·2	12·2	1807 ...	10·1	11·6	1858 ...	54·8	56·3
1757 ...	32·4	31·9	1808 ...	8·1	7·6	1859 ...	93·8	90·3
1758 ...	47·6	47·2	1809 ...	2·5	3·1	1860 ...	95·7	94·8
1759 ...	54·0	54·5	1810 ...	0·0	0·0	1861 ...	77·2	77·7
1760 ...	62·9	64·7	1811 ...	1·4	1·7	1862 ...	59·1	61·1
1761 ...	85·9	80·2	1812 ...	5·0	4·5	1863 ...	44·0	45·4
1762 ...	61·2	60·1	1813 ...	12·2	12·1	1864 ...	47·0	45·2
1763 ...	45·1	48·5	1814 ...	13·9	15·5	1865 ...	30·5	31·4
1764 ...	36·4	36·7	1815 ...	35·4	35·1	1866 ...	16·3	14·7
1765 ...	20·9	21·4	1816 ...	45·8	46·1	1867 ...	7·3	8·8
1766 ...	11·4	14·2	1817 ...	41·1	39·8	1868 ...	37·3	36·9
1767 ...	37·8	35·9	1818 ...	30·4	30·0	1869 ...	73·9	78·6
1768 ...	69·8	66·8	1819 ...	23·9	23·4	1870 ...	139·1	131·8
1769 ...	106·1	103·4	1820 ...	15·7	16·6	1871 ...	111·2	113·8
1770 ...	100·8	98·5	1821 ...	6·6	6·6	1872 ...	101·7	99·7
1771 ...	81·6	86·7	1822 ...	4·0	4·0	1873 ...	66·3	67·9
1772 ...	66·5	65·7	1823 ...	1·8	2·6	1874 ...	44·7	43·1
1773 ...	34·8	39·7	1824 ...	8·5	8·3	1875 ...	17·1	18·9
1774 ...	30·6	27·5	1825 ...	16·6	16·9	1876 ...	11·3	11·7
1775 ...	7·0	8·8	1826 ...	36·3	35·3	1877 ...	12·3	11·0
1776 ...	19·8	21·7	1827 ...	49·7	51·6	1878 ...	3·4	3·9
1777 ...	92·5	92·2	1828 ...	62·5	62·1	1879 ...	6·0	7·7
1778 ...	154·4	151·3	1829 ...	67·0	67·1	1880 ...	32·3	31·6
1779 ...	125·9	123·4	1830 ...	71·0	67·2	1881 ...	54·3	54·4
1780 ...	84·8	89·2	1831 ...	47·8	50·5	1882 ...	59·7	58·1
1781 ...	68·1	66·5	1832 ...	27·5	26·3	1883 ...	63·7	65·4
1782 ...	38·5	38·7	1833 ...	8·5	9·4	1884 ...	63·5	63·3
1783 ...	22·8	22·5	1834 ...	13·2	13·3	1885 ...	52·2	51·3
1784 ...	10·2	10·3	1835 ...	56·9	59·1	1886 ...	25·4	25·1
1785 ...	24·1	26·7	1836 ...	121·5	121·1	1887 ...	13·1	12·6
1786 ...	82·9	81·2	1837 ...	138·3	137·0	1888 ...	6·8	7·0
1787 ...	132·0	128·2	1838 ...	103·2	103·4	1887 ...	6·3	6·3
1788 ...	130·9	133·3	1839 ...	85·8	83·4	1890 ...	7·1	8·4
1789 ...	118·1	117·0	1840 ...	63·2	61·9	1891 ...	35·6	37·7
1790 ...	89·9	90·6	1841 ...	36·8	38·5	1892 ...	73·0	70·0
1791 ...	66·6	67·6	1842 ...	24·2	23·0	1893 ...	84·9	83·7
1792 ...	60·0	59·8	1843 ...	10·7	13·2	1894 ...	78·0	79·1
1793 ...	46·9	47·3	1844 ...	15·0	17·7	1895 ...	64·0	61·5
1794 ...	41·0	38·5	1845 ...	40·1	38·4	1896 ...	41·8	43·1
1795 ...	21·3	24·0	1846 ...	61·5	59·7	1897 ...	26·2	28·1
1796 ...	16·0	15·6	1847 ...	98·5	97·3	1898 ...	26·7	24·6
1797 ...	6·4	6·5	1848 ...	124·3	125·0	1899 ...	12·1	13·8
1798 ...	4·1	4·6	1849 ...	95·9	95·4	1900 ...	9·5	8·8
1799 ...	6·8	6·9	1850 ...	66·5	69·8	1901 ...	2·7	—

The epochs of maximum and minimum as deduced from these figures are given in Table II. They differ from those published by Wolf (No. lvi. of his 'Mittheilungen') only in respect to the maximum of 1805, which by reason of the newly-added observations (made at Kremsmünster) is changed from 1804·2 to 1805·2.

Table II.—Epochs of Sun-spot Maxima and Minima.

Minima.	Weight.	Maxima.	Weight.	Minima.	Weight.	Maxima.	Weight.
1610·8...	5	1615·5...	2	1755·2...	9	1761·5...	7
19·0...	1	1626·0...	5	1766·5...	5	1769·7...	8
34·0...	2	1639·5...	2	1775·5...	7	1778·4...	5
1645·0...	5	1649·0...	1	1784·7...	4	1788·1...	4
1655·0...	1	1660·0...	1	1798·3...	9	1805·2...	5
1666·0...	2	1675·0...	2	1810·6...	8	1816·4...	8
1679·5...	2	1685·0...	2	1823·3...	10	1829·9...	10
1689·5...	2	1693·0...	1	1833·9...	10	1837·2...	10
1698·0...	1	1705·5...	4	1843·5...	10	1848·1...	10
1712·0...	3	1718·2...	6	1856·0...	10	1860·1...	10
1723·5...	2	1727·5...	4	1867·2...	10	1870·6...	10
1734·0...	2	1738·7...	2	1878·9...	10	1883·9...	10
1745·0...	2	1750·3...	7	1889·6...	10	1894·1...	10

Mean date of minima	1744·21	±0·30	year.
Mean date of maxima	1749·37	±0·43	„
Length of the period from the minima	11·141	±0·036	„
Length of the period from the maxima	11·091	±0·053	„
Length of the period in general, or ⅓(max.+2 min.)...	11·124	±0·030	„

From consideration of his tabulations Prof. Wolfer is led to say:—"As concerns the 35-year period discovered by Dr. Lockyer, it is quite certain that the interval between each minimum and the following maximum, or the so-called duration of increase as well as the duration of decrease of the sun-spot curve, shows variations in the different periods that are not accidental and that most probably have a periodic character. But I do not believe that the material used by Dr. Lockyer is sufficient to give the length of the greater period with any certainty, because the whole interval of time that Dr. Lockyer takes into consideration covered only one and a half of these greater periods of thirty-five years each. As soon as I received the memoir of Dr. Lockyer, I at once investigated whether the existence of this suspected period could be traced backward. This attempt was thoroughly justified, because, at least since 1750, the epochs of maximum and minimum are relatively well ascertained and are much more reliable than Dr. Lockyer seems inclined to allow. With the exception of the years 1784 and 1788, which have the weight of 4 ascribed to them in Table II., there is no other epoch whose uncertainty amounts to one-half year. In Table III. you will find the result to which I at that time arrived; column *a* contains the interval of time elapsing between a minimum and the following maximum. These are therefore the numbers which, according to Dr. Lockyer, should follow a 35-year period. These leave no doubt as to their variability; in fact, this variability seems to be much stronger than Dr. Lockyer found from the periods used by him. Apparently they vary to and fro, but a constant period cannot be found therein. The great variation between 1780 and 1835 would in fact rather point to the period of fifty-five years, which was previously suspected by Wolf and would in no case make a 35-year period probable.

"Dr. Halm, of Edinburgh *, has also drawn attention to the variability of the interval between minima and maxima, and in fact this variability is still more plainly brought out by the method of treatment adopted by him, in that he takes account of the ratio of the ascending to the descending part of the 11-year spot-period instead of considering the absolute duration of the ascending portion, as Dr. Lockyer has done. The accompanying Table III. contains the interval of time, *b*, between the maximum and the following minimum, as also the ratio *a*/*b*. The variability of the ratio *a*/*b* is very striking, and shows in general the same curve as do the figures of Table II.; but there is no indication of a regular recurring period, and especially one of thirty-five years.

"Only one result which Dr. Lockyer has brought out is certain, and one which was also demonstrated by Dr. Halm, both from the observed facts and as a consequence of his new theory of the periodicity of the solar phenomena, namely, that the just-mentioned variations of *a* and of the ratio *a*/*b* proceed parallel with the intensity of the development of the spots through each 11-year period in that a maximum follows a minimum quicker in proportion as the maximum is higher, or, as Dr. Halm expresses it, 'in the individual spot-periods the maximum occurs earlier in proportion as the development of the spots is more rapid.' Dr. Halm has compared the ratio *a*/*b* with the absolute altitude of the individual maxima, as expressed by Wolf's relative numbers; on the other hand, Dr. Lockyer bases his position on the total sum of the spotted area for each individual period. We attain to the same results if, for comparison, we use the mean annual relative-number for a single 11-year period, namely, the sum total of the annual relative-numbers for the whole period divided by the length of the period; therefore $r_{\text{mean}} = \frac{\Sigma r}{\text{P}}$. The agreement shown by curves formed from the figures in column 6 and column 9 of Table III. with those formed in like manner from columns 3 and 5 is quite remarkable. That the interval between minima and maxima depends intimately upon the intensity of development of the spots during that period would seem, therefore, to be quite certain, according to Lockyer's and Halm's investigations. Both these quantities are subject to variations of an apparently periodic character, but it seems to me that the material actually available does not indicate a somewhat regular periodicity, and that the continued existence of a 35-year period is not yet demonstrated."

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* *Astronomische Nachrichten*, No. 3723.

Table III.—Mutual Relations of Maxima and Minima.

Date of		Interval.		Ratio, $a:b$.	r_{max}.	Σr, min. to min.	Length of period, P, min. to min.	$r_{\text{mean}} = \frac{\Sigma r}{P}$.
Min.	Max.	Max. − min. = a.	Min. − max. = b.					
		years.	years.				years.	
1745·0	1750·3	5·3	4·9	1·08	83	—	—	—
1755·2	1761·5	6·3	5·0	1·26	80	471·6	11·3	42
1766·5	1769·7	3·2	5·8	0·55	103	533·0	9·0	59
1775·5	1778·4	2·9	6·3	0·46	151	615·8	9·2	67
1784·7	1788·1	3·4	10·2	0·33	133	840·9	13·6	62
1798·3	1805·2	6·9	5·4	1·28	47	281·6	12·3	23
1810·6	1816·4	5·8	6·9	0·84	46	238·0	12·7	19
1823·3	1829·9	6·6	4·0	1·65	67	394·7	10·6	37
1833·9	1837·2	3·3	6·3	0·52	137	653·9	9·6	68
1843·5	1848·1	4·6	7·9	0·58	125	691·8	12·5	55
1856·0	1860·1	4·1	7·1	0·58	95	548·7	11·2	49
1867·2	1870·6	3·4	8·3	0·41	132	617·3	11·7	53
1878·9	1883·9	5·0	5·7	0·88	65	382·8	10·7	36
1889·6	1894·1	4·5	*7·4	*0·61	84	*458·8	*11·9	*39

Astronomy and Cosmical Physics at the British Association †.

At the Bradford meeting, in 1900, a department of astronomy was established as a subsection of Section A, in deference to a wish that had been expressed that astronomy should be better represented at the British Association. After two years' experience it was felt that the astronomical papers that were received were scarcely enough to justify this division of Section A, and this year the subsection was strengthened by the addition of cosmical physics. On the Friday the whole section was given up to cosmical physics; on the Monday and Tuesday the subsection met separately. On all three days the room was well filled, and it was apparent that the enlarged subsection was in some ways a success. But towards the end of the meeting there was a strong expression of opinion that it is not to the advantage of the section as a whole that it should be so much subdivided, and the whole matter will have to be carefully considered before next year. Perhaps a solution of the difficulty might be found in the division of all papers for Section A into two classes—papers that are generally intelligible and papers that are not—and in relegating the latter class only to subsectional meetings. It is not impossible that this might have beneficial results in more ways than one.

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* These figures depend on assumed values of the date and the r for the minimum of 1901·5.

† Communicated to *Nature*, Oct. 23, by Mr. Hinks, Secretary of the Subsection.

The address of the chairman, Prof. Schuster, has already been printed. His criticisms of mere routine observation raised an interesting discussion, in which the meteorologists reserved their defence.

Among the papers submitted to the subsection were several of general interest. Major S. G. Burrard, R.E., described the difficulties which are caused in the Geodetic Survey of India by the attraction of the mass of the Himalayas and the Thibet plateau, and by the existence of an underground chain of excessive density which runs across India. Prof. Turner described an attempt made at Oxford to verify the suggestion put forward by Sir David Gill that the bright stars, as a whole, are rotating relatively to the fainter stars. From the Oxford astrographic catalogue plates he finds distinct evidence of such an effect, but with a sign opposite to that found by Gill in the southern hemisphere.

The feature of the meeting on September 16 was an exhibition of photographs from the Yerkes Observatory. Mr. Ritchey has made a most interesting set of pairs of photographs of star-clusters, made respectively with a 2-foot reflector and with the 40-inch visual refractor fitted with a colour-screen used in contact with isochromatic plates. With the latter the densest parts of the clusters are beautifully resolved and measurable. The photographs of nebulæ made with the 2-foot are unsurpassed; and it is remarkable how, though nearly all the detail which they show *can be found* on the photographs taken at Crowborough and Daramona and Greenwich, the general effect is in some cases quite different. Mr. Percival Lowell sent three papers, one describing a scheme for sending expeditions in search of good "seeing." Mr. S. A. Saunder discussed the possibility of changes in the surface of the Moon, and urged the need of co-operation in the work of describing minute detail. Mr. W. E. Wilson reported failure in his search for Forbes's hypothetical extra-Neptunian planet, and showed a bolometer mounted equatorially for measuring solar radiation.

A. R. H.

Some interesting Double Stars.

[Continued from p. 301.]

Σ 2900, 33 *Pegasi.*—This is a triple star, the magnitudes being 6·0, 7·9, and 9·2. The distance between A and C is 66″·6 and is decreasing. From a discussion of the measures of this pair down to 1874, Otto Struve derived the following equations:

$$\Delta\,.\,\text{R.A.} = -24''\cdot00 - 0''\cdot314\,(t - 1855\cdot04)$$
$$\Delta\,.\,\text{N.P.D.} = -54\ \cdot35 - 0\ \cdot008\,(t - 1855\cdot04)$$

These are practically in agreement with those derived by myself from measures down to present time, viz.:—

$$\Delta\,.\,\text{R.A.} = -38''\cdot60 - 0''\cdot330\,(t - 1900$$
$$\Delta\,.\,\text{N.P.D.} = -54\ \cdot50 - 0\ \cdot004\,(t - 1900)$$

where the annual motion is shown by the coefficient of t. Dr. Auwers gives the Proper Motion of A as—

R.A. +0″·317 and N.P.D. +0″·019.

Consequently the relative motion of the pair AC is due to the Proper Motion of A.

With respect to the motion of the pair AB, a discussion of the micrometer measures shows that the motion of B relative to A is not uniform, and hence it would be fair to assume the pair to be a binary system; but as the motion only amounts to

0″·020 in position-angle 1°,

it is clear that B partakes of the large Proper Motion of A, and AB is certainly a binary system, the plane of the orbit being very nearly coincident with the plane passing through the line of sight. The following are representative measures:—

1832.	Pos.-angle	181°,	distance	2″·85
1863.	„	180°,	„	2 ·35
1900.	„	178°,	„	1 ·54

The position for 1900 is R.A. 22ʰ 18ᵐ 51ˢ, N.P.D. 69° 39′.

Σ 2944, *Bradley* 3011.—This is also a triple star, the magnitude being A = 7·0, B = 8·2, C = 7·5, and the position of C is position-angle 130° and distance 47″·5. The relative motion of C with respect to A is 0″·380 in 37°, or

+0″·229 in R.A. and −0″·303 in N.P.D.;

while the Proper Motion of A, according to Auwers, is

−0″·205 in R.A. and +0″·308 in N.P.D.,

and of C, according to the Dunsink observations, is

−0″·100 in R.A. and +0″·009 in N.P.D.,

a large discrepancy, but with which we are not concerned, the real interest being centred in B, which has a relative motion with respect to A of 0″·017 in position-angle 38°, or

−0″·012 in R.A. and +0″·008 in N.P.D.,

and hence partakes of the large Proper Motion of A, the system being therefore binary. It is an easy pair:

1822·90	245°·6	4″·35
1899·78	256°·3	3″·11

The position for 1900 is R.A. 22^{h} 42^{m} 41^{s}, N.P.D. 94° 45′.

The star C is W.B. xxii. 850.

Σ 3041, *Lalande* 46620.—This is also a triple star and very interesting. The principal star (A) is 7·3 magnitude, while B, which is 65″ distant, is 8·1 mag. The relative motion of B with respect to A, as derived from micrometer measures, is 0″·143 in position-angle 150°, or

+0″·092 in R.A. and +0″·109 in N.P.D.,

B being situated, with respect to A, in the 4th Quadrant, and the distance gradually diminishing would ordinarily mean that A the larger star was, from its proper motion, running towards B.

Romberg, however, gives for the Proper Motion of A:

+0″·050 in R.A. and +0″·070 in N.P.D.,

which would give B a Proper Motion of

+0″·142 in R.A. and +0″·179 in N.P.D.,

a case of the smaller star having the larger proper motion. Now situated in position-angle 176°, and at a distance of 3″·3 from B, is another star of the 8th magnitude, which relative to B has a motion of

+0″·003 in R.A. and −0″·006 in N.P.D.

This star, therefore, must partake of the large Proper Motion of B, and BC is probably a binary system worth attention. A apparently does not belong to the system.

The position is R.A. 23^{h} 42^{m} 46^{s}, and N.P.D. 73° 29′.

Σ 3050, 37 *Andromedæ.*—This star has been under observation since 1792; the first position, however, on which we can rely is 1830·7, 189°·1 and 4″·07, by Dawes; the present position being 1901·1, 214°·4 and 2″·55, by Mr. Bowyer, indicating a rectilinear motion of 0″·026 in 245°. The proper motion, according to the Greenwich 1890 catalogue, is

+0″·106 in R.A. and −0″·049 in N.P.D.,

being much larger than the relative motion, and the pair must be kept on our observing lists because there is always the possibility of its being binary. The distance should never get less than 2″·2 if the motion is rectilinear. The magnitudes are 6·0 and 6·0, and the position R.A. 23^{h} 54^{m}, and N.P.D. 56° 50′. T. L.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Red Spot Region of Jupiter.

Gentlemen,—

Having been absent from home since five weeks, I made no observations since September 8, and it is but to-day that I read the letters of Messrs. Denning and Phillips in your last number. These latter caused me to revise my observations of the pyramid spots, and I arrive at the following conclusions:—

When I *first* measured the pyramid spot on 1901 August 1, it consisted of but *one* pyramid, whose *p.* end had the longitude 225°, and the *f.* end 243°, so that the whole extension comprised 18 degrees, and the centre stood under 234°. My *last* measure of that marking in 1901 was on October 12, when it consisted of *three* pyramids, whose preceding end had $\lambda = 183°$, the following 213°, consequently the centre 198°, and the whole extension 30 degrees. The *p.* end was afterwards measured on October 19, 1901, and found 174°, and on October 24, when I found it 168°. But in both cases I noted in the journal, "not very reliable."

It is not easy to compute the movement of the pyramid spot because it is a matter of fact that there was first but *one* pyramid, later *two*, and finally *three*, so that it is sure that there have been breaking out *new* formations in addition to the first marking. Now it is difficult to decide whether the new markings formed themselves on the *following* or on the *preceding* end of the spot. Let us consider the *centre* of the whole formation as the most reliable point for computation; we find that the pyramids travelled from August 1 till October 12 (72 days) from $\lambda = 234°$ to $\lambda = 198°$ (36 degrees), consequently *just* $0°\cdot5$ *a day*. With this rate of speed the pyramids ought to have had their centre at 73° on June 19, 1902, when Mr. Phillips observed them extending from the *f.* shoulder for more than 35 degrees. The *f.* shoulder was measured by me several times during the present opposition, and may be taken as lying under $\lambda = 62°$, so that the pyramids must have extended from 62°–98°, their centre being under 80°. This corresponds astonishingly well with the movement stated above, particularly if one considers that the Great Red Spot must have opposed an obstacle to the advance of the pyramids. In this way the difference between the computed λ of 73° and the observed one of 80° would be justified, even neglecting the fact that the extension of the pyramids had increased by 6 degrees, which gives suspicion that a *fourth* pyramid had joined the ancient ones. Under such an assumption the observed and the computed position would perfectly agree.

But now a great difficulty offers itself. Nine days later (June 28) Messrs. Denning and Phillips find a spot under $\lambda = 19°$. They believe that it is identical with the same spot they saw nine days before under 80°! Can one believe that a spot which moved during a year with a mean velocity of 0°·5 a day should change the latter in such a degree that it makes *nearly seven degrees a day*? and that in spite of the obstacle of the Great Red Spot it must have passed in any way!

That is an impossibility! But I found out the explanation of the strange confusion caused by the appearance of the supposed pyramid spot on the *p.* side of the Great Red Spot. On 1902 July 30 I saw a marking I supposed to be the ancient pyramid spots just close to the *f.* shoulder; on August 13 I saw it above the latter. On August 25 I was astonished to find not only a dark spot still above the *f.* shoulder (besides the common "bridge") but also *three pyramids left to the Great Red Spot*, upon which there was dark material. The *p.* end of the first pyramid was measured to 341°, the top of the second pyramid to 1°, and the *f.* end of the third pyramid (corresponding to the *p.* shoulder) to 26°. On August 30 I found the pyramids advanced, the spot above the *f.* shoulder disappeared, and the "bridge" in its ancient form; on September 4 the same observation; and on September 8 I was able to measure the *p.* end of the first pyramid to 332°, the *f.* end of the second pyramid to 356°, and the *f.* end of the third pyramid to 26°. The second and third pyramids were separated by a void space of about 25°.

These observations prove that the movement of the pyramids is not a uniform one, the third having been left behind. Besides, it must be mentioned that on my drawing of August 13 I find a pyramid spot under about 8° and two dark spots *upon* the Great Red Spot. Now, if we suppose that the general movement of the pyramids was of 0°·5 a day, we may accept it as certain that the spot seen under $\lambda = 8^\circ$ on August 13 was identical with the second pyramid of August 25, whose centre was then under $\lambda = 1^\circ$, against 2° according to the computation. It is therefore sure that the first pyramid broke out *after* August 13, while it may be that the third pyramid of August 25 was identical with the dark spot *upon* the Great Red Spot which stood there under 40°.

Considering the observations of Messrs. Denning, Phillips, and myself, it is evident that neither the markings seen by themselves on June 28 nor the pyramids seen by myself in August and September can be identical with the pyramid-spots of last year. These latter were really under the longitudes of the Great Red Spot in August 1902, and most probably identical with the dark spots seen by me partially *upon*, partially *around* the Great Red Spot. The spots seen by the English observers on June 28 under 19° and in September to the left of the *p.* shoulder are *new* formations, and identical with my own spots seen in August and spoken of in the above lines. If there be any doubt it is removed by the observations of Señor Comas Solá, published in the B.S.A.F., where one sees above the *p.* shoulder two spots on June 25 and three on July 6, although there are still four spots between about 74° and 96° on June 25 and three spots between 62° and 80° on July 6 in the drawings. It is evident that the latter spots are identical with those seen by the English observers on June 19 under 80°, while the other spots are *new* ones.

So I was perfectly right in saying that the spots of June 28 had nothing to do with the pyramid-spots of 1901.

Yours faithfully,

Manora Observatory, 1902, Oct. 18. LEO BRENNER.

Johann Bayer.

GENTLEMEN,—

As next year will be the tercentenary of the publication of Bayer's 'Uranometria,' the convenience of the nomenclature of which led to its universal acceptance, a few words may not be amiss with regard to the author, particularly as several mistakes have been made in the matter. In Poggendorff's 'Biographisch-literarisches Handwörterbuch' the dates of his birth and death are left blank, as if they were not known. In Larousse's 'Grand Dictionnaire Universel' we are told that he died in 1660, which, as the 'Uranometria' appeared in 1603, would make him attain a great age, which he did not. The true dates of his birth and death are given in Meyer's 'Konversations-Lexikon.' He was born at Rhain, in Bavaria, in the year 1572, and died on the 7th of March, 1625, at Augsburg, where he had long practised as an advocate. It is remarkable that that Lexicon falls into the same mistake which was pointed out in the 'Penny Cyclopædia,' that Bayer was ennobled by the Emperor Leopold I., who was not *born* until 1640, fifteen years after Bayer's death. This is also stated in the 'Biographie Universelle,' and must have arisen from confusion with another person of the same name.

That Larousse should adopt the common error of saying that Bayer designated the stars in each constellation "d'après l'ordre de leur grandeur" is perhaps not strange; but it is more remarkable that the careful and learned writer in the 'Penny Cyclopædia' should also say that "he seems to have arranged the stars in order of brilliancy." On this subject you did me the honour of publishing communications * some time ago; it is only necessary to look at his maps to see at once that, after arranging the stars in groups of brightness, he made the letters of each group follow, not their gradations of brightness, but the shape of the figures the constellations were supposed to resemble. This first occurred to me whilst looking at Ursa Major; and perhaps it may have recently occurred to others whilst looking for Perrine's new comet and noticing the sequence α–ϵ of the stars of Cassiopeia. Prof. Pritchard remarked that in this matter Bayer "missed a great opportunity"; but surely it was merely a case of shrinking from the very considerable labour involved, and which was so well performed at Bonn about sixty years ago by Argelander, whose 'Uranometria Nova' appeared in 1843, or two hundred and forty years after Bayer's 'Uranometria.' Delambre's remark that Bayer obtained his celebrity at very small expense (" à bien peu de frais," *Discours préliminaire* to the 'Histoire de l'Astronomie moderne') is well known; he also says ('Histoire,' vol. ii. p. 185) that " l'ouvrage et l'explication sont également médiocres." Nevertheless the idea of the nomenclature (though something similar had been suggested by Piccolomini) was a happy one, and as all have followed the scheme, we cannot but honour the memory of the Augsburg lawyer and astronomer.

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* In vol. viii. of the *Observatory*.

Yours faithfully,

Blackheath, 1902, Oct. 14. W. T. Lynn.

The Use of the Réseau.

Gentlemen,—

I do not know whether it lies within the scope of your scheme for securing to stay-at-home astronomers some of the advantages of foreign travel, to allow them to ask in your columns some of the questions which they would like to ask if they were abroad. Since asking questions is one way of obtaining opinions, I venture to put to your readers this question: Why has the *réseau* not been adopted in the United States by those observatories that make photographs for measurement?

The use of the *réseau* was made binding on all observatories that had a share in the Astrographic Chart, to serve as a control on the distortion of the film. Its value for this purpose has perhaps been somewhat discounted by the discovery that distortion of the film is not so great as had been feared. But the *réseau* makes measurement so easy; it is so great an advantage to have the standard scale impressed on the plate instead of mounted separately on the measuring machine; it is so much more simple to measure short distances than long, that in all countries, I believe, except the United States, every star photograph intended for exact measurement has a *réseau* upon it. It seems to me that it would be very interesting to know what are the disadvantages in the *réseau* which have led American astronomers to reject it, and I venture to suggest it as a case on which interchange of opinion would be of value.

Yours very faithfully,

Cambridge, 1902, Oct. 17. Arthur Hinks.

The Total Lunar Eclipse.

Gentlemen,—

Between $3^h\ 25^m$ and $4^h\ 25^m$ on the morning of October 17 the sky here was involved in heavy clouds drifting across from N.N.W., but after a light shower of rain the firmament cleared beautifully, and from $4^h\ 25^m$ to $5^h\ 38^m$ A.M. the progress of the eclipse was uninterruptedly watched. One of the most striking features of the event consisted of a broad dark band stretching across the eclipsed Moon from E. to W., and dividing the two intensely red-coloured limbs N. and S. A few minutes before the commencement of the total phase, and when only a segment of the W. edge remained brightly illuminated, the aspect of our satellite presented a notable similarity to the telescopic appearance of Mars when "the Hour-glass" (Syrtis Major of Schiaparelli and Kaiser Sea of Green) is central. When totally eclipsed, the Moon remained pretty distinctly visible to the naked eye (the opacity of the shadow being little, if any, more intense than usual), but was growing perceptibly fainter in the increasing twilight when, at 5^{h} 38^{m} A.M., she sank into a bank of cloud above the W., and was not seen again.

Yours faithfully,

Bristol, 1902, Oct. 21. W. F. DENNING.

OBSERVATORIES.

[Continued from p. 373.]

MÜNCHEN. *H. Seeliger.*—The observation catalogue contains the B.D. stars to 7·0 mag. in the zones +40° to +45° and +55° to +60°, and any other stars in the zones with known proper motions. With the 4½-inch Zeiss aplanatic 460 plates were exposed on 38 separate portions of the sky. The 10½-inch refractor was employed by Dr. Villiger on Saturn and in determining with the aid of a Töpfer photometer the surface brightness of Nebulæ. Observations were also made of Nova Aurigæ (13·5 mag.). A long series of observations of Nova Persei by Dr. Oertel are published in the *Astronomische Nachrichten.*

O'GYALLA.—Variable stars, particularly β Lyræ, η Aquilæ, δ Cephei, η and ζ Geminorum, χ Cygni, S Sagittæ, T Vulpeculæ, R Serpentis, R Coronæ, R Cygni, S Cephei, and Nova Persei. Observations of the Sun's surface were continued—free from spots on 143 days. Meteors. Time-service.

POTSDAM. *H. C. Vogel.*—The 80 c.m. refractor was employed in spectrographic work, 122 spectrograms of stars being secured, of which 26 were of Nova Persei. A fourth set of 25 were obtained of Polaris. With the 32·5 c.m. refractor 375 star-spectra were obtained, amongst them being ζ Ursæ Majoris, χ Cygni, α Boötis, α Canis Minoris, and α Persei. The fine series of ζ Ursæ Majoris have been measured (see 'Sitzungsberichte der kön. Akademie der Wissenschaften zu Berlin,' 1901, xxiv.). Dr. Eberhard published those of χ Cygni in A.N. 3765. Prof. Müller and Prof. Kempf continued their photometric work for the Durchmusterung, observing about 3000 stars. Measures of the star chart plates were not continued, the work being: (1) computation of coordinates for 94 plates; (2) cataloguing 12 plates of 2634 stars; (3) computation of approximate positions of 1631 stars on 8 plates; (4) identifying with the B.D. some 13,000 stars. The third volume of the Catalogue was in the printer's hands in January. Dr. Scheiner worked on the comparisons of magnitude with the B.D., remeasuring all stars showing large discrepancies for the B.D., comparison of approximate places whereby errors larger than 1^s in R.A. and $0''.5$ in Declination would be detected. Time-service.

STOCKHOLM. *K. Bohlin.*—The astrophotographic camera was in use for systematic work on the brighter nebulæ G.C. 1532, 3258, 4373, 4964, and Webb. ($21^h\ 3^m.2$, $+41^\circ\ 50'$). Exposures of from 5 to 8 hours were also made on Nova Persei, the nebula around the star being well shown.

STRASSBURG. *E. Becker.*—The large refractor was in use for Eros, Comet 1900 III., and Encke's Comet. The observations with the Meridian Circle were, besides those of Polaris and δ Ursæ Min.:—

Fundamental Stars, observations in R.A. 2305, in Dec. 603.
Circumpolar Stars „ R.A. 2264
Eros Stars „ R.A. 834

and a large number of observations of the larger planets.

UTRECHT. *A. Nijland.*—32 observations of Mira Ceti, 41 of Nova Persei, a few of other variables. No Lyrids were seen and only 59 Leonids. Positions of Eros, Diana, Fortuna, and other small planets. Eclipse expedition to Karang Sago under Dr. Julius most successful.

WIEN (*M. Edler v. Kuffner*). *L. de Ball.*—Zone work. Double stars. Prime vertical observations.

ZURICH. *A. Wolfer.*—Solar work as in previous years, the Sun's surface being examined on 291 days, and prominences on 169 days. The following table gives the comparison of the two years:—

1900. Observing days 276, rel. no. 8·8; no spots 118 days.
1901. „ 291, „ 2·6; 230 „

NOTES.

COMET NOTES.—Perrine's Comet has been easily visible to the naked eye when the Moon was absent in October. Its total light was probably at least as great as that of a fourth-magnitude star. It was not, however, quite so conspicuous an object as at one time seemed likely; possibly it may make a better display to southern astronomers after perihelion passage. Nijland's elements given last month are still the most accurate that have yet appeared;

there was, however, a misprint in the value of ω which should be corrected. It should read 152° 53′ 2″ instead of 132°.

On Sept. 28 Nijland's R.A. was too great by 22^s, his Dec. too great by $0'.2$.

On Oct. 8 Nijland's R.A. was too great by 29^s, his Dec. too great by 6′.

The comet may still be seen early in November, but it will be very low down and must be looked for as soon after sunset as possible; an ephemeris and map of its path were given last month.

Search has been made for Grigg's Comet (*c* 1902) on the Harvard photographs, but without success. Mr. Grigg was therefore the only person who observed it. Application has been made to him for as full information as he can give, and possibly when it arrives it may be sufficient to deduce a tolerable approximation to the orbit. A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Magnitude.
JT	Sept. 26	Carnera.	12·7
JU	Oct. 7	Wolf.	10
JV	,,	,,	12
JW	,,	,,	13
JX	,,	,,	13
JY	Oct. 24	,,	13
JZ	,,	,,	13
KA	Oct. 25	Dugan.	13

JU is unusually bright for a new planet. The planet designated JN proves to be identical with 311 Claudia.

Later. JU proves to be identical with 106 Dione. A. C. D. C.

THE METEOROLOGICAL CONDITIONS OF THE SUMMER OF 1902.—The distinctive feature of the past summer may be considered to have been its cold and comparatively sunless character. The total amount of sunshine recorded at the Royal Observatory in the five months May to September (inclusive) was 812 hours, being 207 hours less than the average for the years 1897-1901, 306 hours less than the amount recorded in 1899, and 238 hours less than that recorded in 1901. There were only seven absolutely sunless days in these months, so that it will be seen that the amounts recorded on the 146 days of Sun register (averaging 5½ hours daily) were usually small. The largest daily amount was 15·4 hours on June 27, and the largest monthly amount 201 hours in July. With the exception of a few short periods of sustained warmth in June and July, aggregating about 20 days on which the mean temperature exceeded the average by 4° to 5°, the season was very cold throughout. The mean monthly temperatures were:—May, 48°·4; June, 57°·6; July, 60°·7; August, 59°·4;

September, 55°·9, which were all lower than their respective averages. There were 54 days on which the thermometer rose above 70°, 9 of these exceeding 80°, the highest reading having been 86°·1 on July 14; but, on the contrary, there were 38 days on which the maximum temperature did not exceed 60°, the lowest recorded maximum being 48°·0 on May 20. On the morning of May 14 a maximum temperature of 29°·8 was recorded, so that in two months the extreme range of temperature amounted to 56°·3. The thermometer on the grass fell below freezing-point on 15 days in the month of May, on one day in each of the months of June and July, and on six days in September, the lowest reading of that thermometer having been 22°·6 on May 14. The rainfall amounted to 12 inches, being one inch greater than the average fall. An interesting tabular comparison of some of these results may be made with the results for the cold years 1845, 1860, and 1879.

TABLE I.

Years (May to September).	Daily Maximum Temperatures.				
	Above 70°.	Above 80°.	Below 60°.	Highest.	Lowest.
				°	°
1845 …………	51	6	28	86·0	50·7
1860 …………	28	0	17	76·5	55·0
1879 …………	30	1	22	80·6	47·6
1902 …………	54	9	38	86·1	48·0

TABLE II.

Years (May to September).	Temperature.				Rainfall.
	Mean Maximum.	Mean Minimum.	Mean Daily Range.	Mean.	
	°	°	°	°	in.
1845 ……………	66·9	49·2	17·7	56·2	11·17
1860 ……………	66·1	48·2	17·9	56·1	19·28
1879 ……………	65·5	48·7	16·8	56·1	19·43
1902 ……………	66·2	48·3	17·9	56·4	12·10
50 years' average.	69·8	49·8	20·0	58·8	11·09

The years 1860 and 1879 were much wetter than the years 1845 and 1902, but in other respects the similarity between the results is sufficiently remarkable. No comparison can be made of sunshine results, as this element was not made a subject of record in the earlier years. W. C. N.

FOUCAULT'S PENDULUM.—This well-known experiment to exhibit the rotation of the Earth is now being performed daily at Paris in sight of a large and more or less intelligent crowd, to whom a popular lecture is delivered on the subject on Sundays and Thursdays. A ball of lead 28 kilogrammes in weight, exactly similar to that used by Foucault in his experiment in the same place, is suspended by a piano wire 67 metres long and 72 mm. in diameter from the top of the interior of the dome of the Pantheon. It is set swinging by the usual method of the burned thread in a north-and-south plane over a graduated table, and an attached point passes through small heaps of sand at either end of the swing. One of the Editors of this Magazine, who has had the opportunity of seeing this, is able to report that the plane of swing did apparently move, with respect to the table, clock-wise at the rate of about four degrees in 20 minutes.

MR. F. W. HENKEL, who was for a short period on the staff of the University Observatory, Oxford, and had charge of Col. Cooper's Observatory at Markree, Ireland, would be glad to hear of a vacant post in any observatory, or would undertake work of a scientific nature.

THE observations for the re-determination of the difference of longitude Paris—Greenwich are coming to an end. M. Bigourdan and Mr. Dyson completed their series of observations at Paris on October 22, and at the date of writing this (October 30), M. Lancelin and Mr. Hollis are observing at Paris, wanting only two more nights of simultaneous observations at the two stations to finish the work.

THE first Meeting of the Session of the Royal Astronomical Society will take place on November 14, at 5 o'clock. The next Meeting of the British Astronomical Association will be on Wednesday, November 26.

FROM AN OXFORD NOTE-BOOK.

THE Supplementary Number of the *Monthly Notices* contains a remarkable paper by our Secretary, Mr. E. T. Whittaker. During the Recess he discovered the general solution of Laplace's equation, which has stood as a challenge to mathematicians for more than a century; and, as though this were not a sufficient achievement, he has been led, by connecting this equation with that for the radiation of "normal" waves (such as those of sound), to a conception of gravity as a complex wave-motion—as an "interference" phenomenon, in fact. There are great and obvious difficulties in the physical interpretation of this conception, the clearing up of which we may await with patience; but the conception itself is a most beautiful and novel one, and the mathematical work which led up to it is of the very first quality. Although the paper is already in print, we shall hope to hear Mr. Whittaker say something about it at the November meeting; and those who have heard him read a paper of any kind know that he is able to give an interesting and intelligible account of complex and technical matters.

Mr. Moulton's book on Celestial Mechanics * is a very welcome addition to our text-books. We have long wanted a book in English of this kind, and the present one is written and printed most attractively. It aims at being interesting rather than complete; and covers a good deal of ground in consequence. But it is pleasant to find the newest things on the "problem of three corpses" put into such easily readable form, side by side with a sketch of methods for computing orbits on the one hand, and a few words on Helmholtz's contraction theory of the Sun on the other. There is one recent paper to which I should like to draw Mr. Moulton's attention; or rather two—viz., those in *Mon. Not.* lvii. pp. 118–133. I think they would have made one chapter in the book a little simpler. But I may add that my special knowledge of these papers is not due to such a comprehensive knowledge of the subject as would entitle me to criticize Mr. Moulton in the ordinary course, but to the fact that I had to correct one of them for press (the author being absent in India), and I think the proof contained more misprints to the square inch than any proof I have ever seen.

Last month, in commenting on Professor Schuster's Address at the British Association, it was remarked that his views with reference to meteorology were "at least as old as De Morgan, as readers of the 'Budget of Paradoxes' will recognize." Unfortunately this great work is getting so scarce that the number of readers may fall considerably short of the number of would-be readers. It may therefore not be superfluous to quote one or two of the passages referred to. De Morgan is vigorously attacking the ideas of Bacon about induction—that you had only to collect plenty of facts, and conclusions must come out of them. On p. 54 occur the following passages :—

---

* An Introduction to Celestial Mechanics. By F. R. Moulton. New York: the MacMillan Co., 1902.

> There is an attempt at induction going on, which has yielded little or no fruit, the observations made in the meteorological observatories. This attempt is carried on in a manner which would have caused Bacon to dance for joy; for he lived in times when Chancellors did dance. Russia, says M. Biot, is covered by an army of meteorographs, with generals, high officers, subalterns, and privates with fixed and defined duties of observation. Other countries have also their systematic observations. And what has come of it? Nothing, says M. Biot, and nothing will ever come of it: the veteran mathematician and experimental philosopher declares, as does Mr. Ellis, that no single branch of science has ever been fruitfully explored in this way.
>
> * * * * * * * * * * * * *
>
> In the report to the Greenwich Board of Visitors for 1867, Mr. Airy, speaking of the increase of meteorological observatories, remarks: " whether the effect of this movement will be that millions of useless observations will be added to the millions that already exist, or whether something may be expected to result which will lead to a meteorological theory, I cannot hazard a conjecture." This *is* a conjecture, and a very obvious one: if Mr. Airy would have given 2¾*d.* for the chance of a meteorological theory, formed by masses of observations, he would never have said what I have quoted.

But to mitigate the severity of this attack on Meteorology and Meteorologists, De Morgan reminds us how easily the same might have been said of Astronomy but for the search-light of Newton's *hypothesis*. On p. 56 he writes :—

> He deduced the ellipse, he proceeded to deduce the effect of the disturbance of the Sun upon the Moon, upon the assumed theory of *universal* gravitation. He found result after result of his theory in conformity with observed fact; and by aid of Flamsteed's observations, which amended what mathematicians call his *constants*, he constructed his lunar theory. Had it not been for Newton, the whole dynasty of Greenwich astronomers, from Flamsteed of happy memory, to Airy whom Heaven preserve, might have worked away at nightly observation and daily reduction, without any remarkable results: looking forward, as to a millennium, to the time when any man of moderate intelligence was to see the whole explanation. What are large collections of facts for? to make theories *from*, says Bacon; to try ready made theories *by*, says the history of discovery; its all the same, says the idolater; nonsense, say we!

We may profitably ponder on such words as these, or on those of Professor Schuster. The temptation to *accumulate material* is a fascinating and powerful one, the exertion of attempting to digest it is often most unattractive. These truths, almost truisms, may be illustrated from almost any walk of life. A great historian has just passed away who spent his life in accumulating such stores of material as never historian probably accumulated before: with the result that " Everybody has felt . . . that he knew too much to write." Alongside the above passages from De Morgan may perhaps be put the following paragraph from Professor Maitland's loyal appreciation of Lord Acton in the *Cambridge Review* for Oct. 16:—

> The immensity of the learning being unquestionable, some disposition to question the use that was—or was not—made of it was to be expected, and has

in fact been observed. That "daily consumption of a German octavo," did it benefit him and the world, or was it only a stupendous feat of intellectual voracity? Reference to the Catalogue of the University Library might give point to the question. One lecture, an inaugural lecture delivered in 1895, one letter written in 1870 (a German letter written to a German bishop)—these, so the enquirer might say, appear to be all the published works of John Emerich Edward Dalberg, first Lord Acton. . . . Was ever such disproportion between intake and output?

Professor Maitland goes on to show, however, that there is another side to this particular picture, which we must not forget.

Rumours reach us that a certain eminent Astronomer ventured recently to make some remarks of a political character. It might be supposed that public events were common property; but this is not the opinion of the Press, or at least of certain organs of it. They were much incensed at the infringement of what they took to be their own particular privileges, and roughly and rudely bade the astronomer "stick to his last," pointing the moral with the following instances of gross neglect of duty on his part:—

> During the past two years there have been but two terrestrial phenomena of any importance from an astronomical point of view in this part of the world, and both escaped the notice of the Astronomer-Royal. One was the comet first discovered by a Piquetberg resident returning home at midnight from the worship of Bacchus, and the other was an earthquake distinctly felt by 10,000 people in this Peninsula, but not recorded at the Observatory. Upon inquiry, the Press learnt that there had been an earth tremor, but the seismeter at the institution was one which only registered north and south, and, as the earthquake in question was from east to west, it had successfully dodged the recording needle of the instrument.

In the notice of the late Dr. C. Wilhelm Schur (an Associate of the R. A. S.) in the Council Report for February last, no reference is made to one of his claims on our gratitude, viz. the preparation of a concise illustrated account [*] of the Göttingen Observatory from 1750 to the present day. The picture of the old Göttingen Observatory and many of the 16 portraits are very quaint:—What a striking resemblance between Joost Bürgi and Galileo! a resemblance which extends in some sort to their dates, viz. 1552–1632 and 1564–1642. On seeing Bürgi's face we read with less surprise the claims made on his behalf—that he invented pendulum clocks before Huyghens, and logarithms before Napier. Next his portrait is that of Tobias Mayer, of whom the following story is told:—During the siege of Göttingen by the French in the seven years' war (1754–1761) he was sent as one of a deputation of burgesses to the beleaguering Commander, who threatened the town with starvation. "A Göttingen Professor is accustomed to starvation," replied Tobias Mayer.

---

[*] Beiträge zur Geschichte der Astronomie in Hannover. Von Wilhelm Schur. (Berlin, Weidmannsche Buchhandlung, 1901.)

From the *Daily Graphic* of October 24, we learn that Foucault's pendulum has been set swinging again in Paris to demonstrate the rotation of the Earth to a large audience, under the appropriate auspices of the Minister for Public Instruction. From the *Cambridge Review* of October 23 we learn that at 5 P.M. on October 24 Mr. Oldham, the University Reader in Geography, was to give an "Experimental Demonstration of the Curvature of the Earth's Surface," illustrated by photographs taken with a telephoto lens. There is an approximation to simultaneity in these two events which is almost startling. Is there an epidemic of disbelief in these fundamental facts, or have we had so much novelty lately that a recurrence to them is considered a necessary and restful change?

The eruptions in the West Indies have not, so far, provided us with any very striking sunset glows; certainly nothing to compare with those following Krakatoa, of which Tennyson has left us the reminiscence in the opening lines of *St. Telemachus*:—

> Had the fierce ashes of some fiery peak
> Been hurl'd so high they ranged about the globe?
> For day by day, thro' many a blood-red eve,
> In that four hundredth summer after Christ
> The wrathful sunset glared against a cross.

But the year has been a failure in several ways: the weather has been continuously bad except at the time when the Coronation was so sadly postponed; and the comet has just been sufficiently conspicuous to be disappointing. Perhaps as a compensation the lost Leonids will turn up in a fortnight's time.

Is it correct, as stated in *Pub. Ast. Soc. Pacific*, xiv. no. 86, p. 174, that (433) Eros is the *only* exception to the rule that the names of the minor planets should be feminine? It is difficult to keep a correct list of these bodies nowadays, but surely No. (342) is named Endymion? and there may be other exceptions, not to speak of doubtful names such as (439) Ohio, and obvious evasions such as (353) *Ruperto-Carola.*

On October 15 last, the measures and reductions of the 1000th plate for the Astrographic Catalogue were completed at the Oxford University Observatory, and a whole holiday was granted in consequence. There are still 180 plates left, but it is hoped to celebrate the completion of the whole before the end of next year.

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T. LEWIS, F.R.A.S.,
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DR. GILL, now Astronomer Royal at the Cape, wrote:—"Of the 50 chronometers used, Kullberg 488 has, on the whole, performed best. Allow me to express the great satisfaction I have had in all the chronometers of your make in the expedition to Mauritius" (Lord Crawford's Transit of Venus Expedition, 1874).

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 November 14.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. One hundred and sixty-four presents have been received since the date of the last Meeting. Among those which may be specially mentioned are:—The 4th edition of Miss Clerke's 'History of Astronomy,' presented by Messrs. A. and C. Black. 'Astronomy without a Telescope,' by Mr. E. W. Maunder, from Messrs. Witherby. 'Miscellaneous Papers of Oxford University Observatory, 1899–1900,' sent by the Observatory. Prof. C. A. Young's 'Manual of Astronomy,' presented by the Author. Photographs of Perrine's Comet taken at Greenwich, presented by the Astronomer Royal; and four photographs of the same comet sent by Mr. R. C. Johnson. A portrait of John Goodricke from Mr. W. T. Lynn; and Mr. W. H. Wesley has presented photographs of three Presidents of the Society.

A vote of thanks was accorded to the donors

Dr. Isaac Roberts read a paper on "William Herschel's observed Nebulous Regions, 52 in number, compared with Isaac Roberts's photographs of the same regions, taken simultaneously with the 20-inch Reflector and with the 5-inch Cooke Lens." He referred to Herschel's paper, "The Construction of the Heavens," published in the 'Philosophical Transactions of the Royal Society' for the year 1811, in which Herschel described fifty-two regions in the sky observed by him to be affected with extensive diffused nebulosity. These regions were tabulated in Herschel's paper, and particulars given concerning each one respectively.

Since then no systematic effort had been made to reobserve these regions until the year 1896, when Dr. Roberts decided to photograph them with his 20-inch reflector and his 5-inch Cooke lens. The photographs were taken simultaneously with the two instruments, and with an exposure of the plates during ninety minutes, when the sky was clear and the object on or near the meridian. The plates of high sensitiveness were selected so that images of stars magnitude 16–17 would be seen on the reflector-plates and of magnitude 14–15 on the 5-inch lens-plates, and thus the stars and nebulosity shown would be at least as faint as those that could be seen by aid of Herschel's telescopes.

The work of photographing the 52 regions was completed this year, and the results of the examination of the photographic plates show that there is no trace of extensive diffused nebulosity on forty-eight out of the fifty-two regions described by Herschel; on the remaining four regions there is nebulosity which forms part of three extensive nebulous clouds, which Herschel could not see in as complete form as they are depicted on the photographs.

Dr. Roberts projected on the screen lantern-slides of two of these objects, which are the nebulosity around ζ Orionis, ♅ V. 37 Cygni, and the great nebula in Andromeda, all three of which are remarkable in structure.

The tabular method adopted by Sir William Herschel in publishing the results of his telescopic observations of these fifty-two regions was also adopted by Dr. Isaac Roberts, and the correlation of the telescopic and photographic observations of these regions is thus given in a concise and intelligible form.

Prof. Turner. I am sure we are all very glad to see the beautiful photographs which Dr. Roberts has placed before us. They are the best that we have had at our Meetings for a long time.

The President. On behalf of the Society I can only express to Dr. Roberts our thanks for bringing before us the result of the beautiful work he has done. He has given us an interesting account of a valuable research, and the result of the comparison has taken me by surprise.

Prof. Turner read a paper on the suggestion made by Sir D. Gill that there is an apparent rotation of bright stars as a whole with reference to faint stars as a whole.

Sir David Gill finds for zone −40° to −52° that bright stars are increasing their R.A. about $+0^{s}{\cdot}0012$ per magnitude per year with reference to faint, suggesting a rotation of bright stars as a whole. This result is arrived at by comparing catalogues made by eye observations from which the effect of "magnitude-equation" has now only partially been eliminated.

Photographs of the same region, taken at dates sufficiently separated, would show this motion without any interference from "magnitude-equation."

The photographs taken at Oxford in zones +25° to +31° for the Astrographic Chart, from 1892 onwards, do not yet allow of this direct method of attack on the problem; but when compared with the Cambridge observations, if the rotation suggested by Sir David Gill exists, the plates taken early should give different results for the Cambridge "magnitude-equation" from those taken later. The effective interval available is only about five years (very small), and the effective range of magnitudes about three. But since this implies a difference of $0''{\cdot}0012 \times 5 \times 3 = 0''{\cdot}018$, the inquiry is not hopeless, even now.

The comparison of a large number of plates gives a quantity about as large as that found by Sir David Gill, *but of the opposite sign.* If this sign is correct the phenomenon cannot be a rotation.

Assuming it correct, we can explain the fact by assuming that the stars of the Milky Way are different in size from others, so different that where the Milky Way appears in (say) the N. hemisphere faint stars, which we take to be more remote than brighter ones, may be really nearer. The effect of the Sun's motion in space would then no longer be symmetrical about its apex, and the disturbance would have opposite signs in the two hemispheres, since the Milky Way appears in belts of R.A. on opposite sides of the sphere.

On comparing the results obtained with Galactic latitudes of the groups we see that this phenomenon of faint stars being on the average nearer than bright takes place from latitudes 25° to 50°, and not close to the Milky Way itself. A simple hypothesis is given in the paper to account for this.

Mr. Dyson (presenting a paper on the same subject from the Royal Observatory, Greenwich). I think the results obtained by Dr. Gill and Prof. Turner should be received with a good deal of caution. A comparison between Groombridge's Catalogue and the Greenwich 1890 Catalogue gives results of the same sign as Dr. Gill's. They are, however, so much smaller in amount that I should hesitate to attribute a consmical explanation to them till the unknown and possibly unknowable magnitude-equation of Groombridge had been allowed for. On the other hand, Prof. Turner's comparison depending entirely on photographic methods avoided that difficulty; but it may be said that in this paper from Greenwich we have a period of 80 years as against 5 years, so that the proper motions should be tolerably well determined. The resulting figures of this paper (which were shown on the screen) compared with those found by Dr. Gill, but they were of a considerably smaller amount and must be taken with a good deal of caution.

The Astronomer Royal. I should like to make an addition to what Mr. Dyson has said. The figures he has shown have not been deduced from the original Groombridge Catalogue, and must be taken as somewhat provisional. It will be of interest to see how far they are modified by the re-reduction which is now being done.

The President. We must thank Prof. Turner for having gone so fully into this matter, and also the Astronomer Royal and Mr. Dyson for the remarks they have made.

Mr. *Whittaker* gave an account of his paper "On the general Solution of Laplace's Equation and the Equation of Wave-motions, and on an undulatory explanation of Gravity," which had appeared in the Supplementary Number of the *Monthly Notices.* The investigation from which the paper had developed was a study of the partial differential equations which underlie the theories of light, sound, and gravitation. The greater part of this investigation was purely mathematical, and was therefore being published elsewhere; but some of the results were of interest in Celestial Mechanics, and these were given in the present paper.

The first result is the general solution of the partial differential equation, which is satisfied by the potential of any gravitating system. This equation was first given by Laplace in his memoir on the ring of Saturn. The solution is given as a certain definite integral: from this definite integral all the particular solutions known hitherto can be derived.

A similar result is then found for the partial differential equation, which is satisfied by the quantities specifying any form of wave-disturbance; and from this is derived an analysis of any arbitrary solution into a sum of elementary terms, each representing a wave-disturbance of the simplest type.

In the last part of the paper it is suggested that this analysis removes difficulties which have hiterto been felt regarding the propagation of gravitation: for each of the elementary terms into which the gravitational potential is thus resolved represents a disturbance which would be propagated in much the same way as the physical phenomena connected with the wave-motion equation. This would involve the acceptance of a finite velocity of propagation for gravity, and also the consequence that Newton's law would cease to hold for bodies travelling with velocities comparable with that of gravitation.

Prof. Turner. I am very glad to have the opportunity of expressing my great admiration for this beatifully simple idea. It is an idea for which physicists have been on the look-out for many years—something to replace the idea of action at a distance in the law of gravitation. Here, for instance, is a little book written by Sir George Stokes 'On the Nature of Light' in 1884, and he mentions this possibility:—

> If the supposition that light consists in undulations obliges us to suppose that space is filled with some kind of substance, at least as far as the remotest star that our most powerful telescopes reveal to us, may it not be that that same substance forms, in some manner as yet unknown to us, the link of connexion whereby the Sun is enabled to attract the Earth, and keep it in its orbit?

On that idea he dwells for a little time. It seems to me that Mr. Whittaker has probably taken a step which is leading us in that direction. There are some difficulties still to be overcome and some objections to be met which, for my own part, I feel confident that Mr. Whittaker will meet in time; and it is only fair to say that the attitude towards this idea of many of our leading physicists is sceptical if not antagonistic. I do not think we need trouble about that, because time will test the hypothesis as it has tested many another hypothesis. We may remember that when the undulatory theory of light was propounded by Huygens it was neglected for nearly a century. The great name of Newton, who advocated the corpuscular theory, overshadowed the undulatory theory, and for more than a century no advance was made. This fact is dwelt upon in this little book by Sir George Stokes; and in concluding his remarks upon it, he writes as follows:—

> It may be said, if the former theory is now-a-days exploded, why dwell on it at all? Yet, surely the subject is of more than purely historical interest. It teaches reasons for our future guidance in the pursuit of truth.
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> It shows that we are not to be disheartened by some preliminary difficulties from giving a patient hearing to a hypothesis of fair promise, assuming of course that those difficulties are not of the nature of contradictions between the results of observation or experiment and conclusions certainly deducible from the hypothesis on trial. It shows that we are not to attach undue importance to great names, but to investigate in an unbiassed manner the facts which lie open to our examination.

So, although it has been very interesting to notice the attitude of several eminent physicists towards the theory of Mr. Whittaker, I shall endeavour to follow the lessons of Sir George Stokes and not attach too much importance to great names. To mention one of the difficulties found: some people find a great difficulty in accepting an infinite integral in order to get the sum total of permanent effect. They think it is stretching things rather far to postulate undulations of every wave-length. But I would remark, in the first place, that in the case of light, particles are emitting light of every wave-length, so far as we know—not only in the spectrum we can see or photograph, but far beyond that. And secondly, this difficulty of being able to accept such "a large order" was just the sort of difficulty people found with the undulatory theory; and I will again quote an example given by Sir George Stokes:—

> The necessity of thus filling space with substance seems to have presented a serious difficulty to some minds. In the course of a conversation with Sir David Brewster, who had just returned from France, where he witnessed the celebrated experiment by which Foucault had just proved experimentally that light travels faster in air than in water, I asked him what his objection was to the theory of undulations, and I found he was staggered by the idea *in limine* of filling space with some substance merely in order that "that little twinkling star," as he expressed himself, should be able to send its light to us.

The difficulty was got over by the mere process of *familiarity* with it; and I confidently expect that we shall in the same way presently get over the difficulty by imagining this infinite integral as the cause of gravity. I look forward with confidence to the future of this very brilliant idea of Mr. Whittaker.

The President. The Society has already expressed its thanks to Mr. Whittaker for the excellent theory he has brought before us. Of course, this Meeting has been mostly occupied by the physical aspect of the matter which is most interesting, but when I saw Mr. Whittaker's paper I was very much pleased with his extremely elegant mathematical solution. When the solution has once been discovered it is not particularly difficult to verify it. I, as a mathematician, certainly congratulate Mr. Whittaker upon having obtained the solution of Laplace's equation, and I confess that I should rather have got this elegant solution of this equation than have done many other things.

The Astronomer Royal then exhibited on the screen photographs of Perrine's Comet, and said:—There is nothing in this paper to call for particular mention, but I thought it would be well to put on record a statement with regard to the photographs of Perrine's Comet which have been taken at the Royal Observatory. They were mostly taken with the 30-inch reflector made by Dr. Common, and they were taken primarily for the purpose of determining the position of the comet. There are 29 of these photographs available for measurement, taken on seven nights. These two photographs, which are now being shown on the screen, were taken respectively with exposures of 62 minutes and 53 minutes. There are altogether seven tails separated by six distinct rifts, and the rifts are more sharply defined than the tails themselves. Mr. Dyson has found that the longest tail is, in all cases, in the direction opposite to the Sun. September 26th and September 29th were the best nights we had for photography, but we have been very much troubled with the bad weather, and have had some difficulty in getting a sufficiently clear night for long exposures, and the sky was not favourable at the time when the comet was at maximum brightness.

Mr. President. Mr. Plummer, you also have a photograph of the comet, have you not?

Mr. H. C. Plummer showed a photograph of the comet taken by Mr. F. A. Bellamy at Oxford with the 13-inch astrographic telescope. The date was September 29, and the exposure half an hour. There was no indication of a tail. Mr. Plummer read a note extracted from the observing-book which recorded the difficulty arising from the illumination, and the unsuitability of the only power with which the instrument was fitted. He also pointed out, in connection with a point to which he wished to refer later, the conspicuous inferiority of the result obtained with the refractor in respect of such work.

Mr. Dyson. We tried to photograph the comet with the 26-inch refractor at Greenwich, but it was barely shown.

The Astronomer Royal. With the reflector we got images with exposures of five minutes to half a minute, the five-minute exposures being made in the earlier stages when the comet was not so bright; whereas with the 26-inch refractor we found that very much longer exposures were required. The observer guided the telescope to follow the comet, and therefore the stars are shown as long trails.

Mr. Dyson said that a communication had been received on this subject from Prof. Max. Wolf, who sent two photographs, which were taken at some little time apart with a view of getting a stereoscopic effect. These will be passed round the room with a stereoscope, and Fellows may notice that the comet appears to stand out in front of the stars.

The President. It is our pleasing duty to return thanks to Mr. Christie, Prof. Turner, and Prof. Max Wolf for the communications they have made to us.

Mr. Wickham. I should like to say that while observing the comet at Oxford on two occasions, I detected that the nucleus was treble. There appeared a centre nucleus which was the largest of the three, and on either side a smaller one, and a line joining these nuclei seemed to run along the tail. I mention this as an interesting point. In the photographs these nuclei seem to be combined, and therefore I think the visual observations have some advantage over the photographic. I may add that this was confirmed by another observer, one of my colleagues who was present with me.

Mr. Dyson then read a paper by *Mr. Percival Lowell* on a standard scale of "seeing," in which the author called attention to the apparent anomaly that, though observers were careful to give the size of the object-glass of their telescope and the focal power they used to make an observation, they scarcely ever made any reference to the state of the sky when the observation was made, and he suggested the adoption of some uniform system by which astronomers might describe the state of the "seeing."

Prof. Turner. I should like to draw special attention to this paper, which is of the nature of a circular, asking observers to consider it. Mr. Lowell is very anxious to get this standard scale generally adopted; and although it would hardly be profitable to discuss it now until the paper has been read and examined by Fellows, I hope they will read it and make some experiments, and then we may have some profitable discussion on it at another time. It would seem to be for the welfare of astronomy generally if some absolute scale of "seeing" could be devised and generally accepted.

Mr. Maw. I should like to be able to read the paper in full before offering any remarks. I am not quite clear what Mr. Lowell desires. The conditions of "seeing" must be considered with reference to the kind of work to be carried on. With regard to double-star work, all that is wanted is a clearly deeply defined disc, and a night which is perfect for double-star work may not be so for other work. I should like to read the paper before making any further remarks, but I do not quite see that there is anything that would meet the object he has in view.

Mr. H. C. Plummer (presenting his second paper on the images formed by a parabolic mirror). This paper gives some results which have been obtained by comparing a plate taken with a reflector with one of the astrographic plates taken at Oxford. As the time available is short, I will read a summary of the main conclusions:—(1) The positions of stars with magnitudes between 7·0 and 10·0 can be measured with a probable error of about 0″·6 in both coordinates. As compared with astrographic plates the degree of accuracy is low, but it is of the same order as that of the A.G. catalogues. (2) Images near the edge of the plate (about 1° from the centre of the field) can be measured as accurately as those nearer the centre. (3) As the magnitude of the stars approaches 11·0 the order of accuracy is fairly maintained, but there is some evidence of change of scale-value. Two methods of setting have been employed throughout [explanation on blackboard], and both have been found to possess advantages and disadvantages. The two photographs used are now shown on the screen. The reflector photograph, for which I am indebted to the kindness of Dr. Isaac Roberts, is a beautiful picture of the nebula of ♅ V. 15 Cygni. The astrographic plate, with its short exposure, bears no trace of the nebula, but the stars can be measured with a far higher degree of accuracy.

Prof. Turner. Although the result is more or less a negative one, it represents a great deal of hard work on the part of Mr. Plummer. He has come to the conclusion that the difference in measures cannot be explained by any systematic error which we can trace to its source and make allowance for. At one time in the course of the investigations it seemed possible that a certain systematic error would have been found which would have rendered the photograph taken with the reflector the more valuble.

Mr. Hinks showed on the screen a pair of photographs of the Cluster in Hercules, belonging to a set of photographs which had been sent to Sir Robert Ball by the Director of the Yerkes Observatory. In the first, made with the 2-foot reflector, the centre of the cluster was not resolved; in the second, made with the 40-inch visual refractor, fitted with a colour-screen in contact with the isochromatic plate, the cluster was clearly resolved and measurable. He also showed two photographs of the Andromeda nebula, also made by Mr. Ritchey with the 2-foot reflector, and pointed out how remarkably they brought out detail which, though it generally could be found when looked for on the photographs made by Dr. Roberts and at Greenwich, was now for the first time shown conspicuously.

The following papers were announced and partly read :—

H. C. Russell. "Another Form of Micrometer for measuring Star Positions."

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon for 1903."

Max Wolf. "Stereoscopic Pictures of Comet Perrine."

H. C. Plummer. "On the Images formed by a Parabolic Mirror." Second paper: "Influence on the Measurement and Reduction of a Photograph."

M. Loewy. "Sur la Précision des Mesures Photographiques."

Isaac Roberts. "W. Herschel's observed Nebulous Regions, 52 in number, compared with Isaac Roberts's Photographs of the same Regions, taken simultaneously with the 20-inch Reflector and the 5-inch Cooke Lens."

H. H. Turner. "On the Suggestion made by Sir D. Gill that the Brighter Fixed Stars are as a whole rotating with respect to the Fainter Stars as a whole."

Royal Observatory, Greenwich. "Note on Perrine's Comet." (Communicated by the *Astronomer Royal.*)

W. G. Thackeray. "Newcomb's Fundamental Catalogue: Errata and Notes."

Royal Observatory, Greenwich. "Note on a Comparison of Groombridge's Catalogue (1810) with the Greenwich Second Ten-Year Catalogue (1900) with Reference to the Question of an Apparent Rotation of the Brighter Stars as a whole with respect to the Fainter Stars." (Communicated by the *Astronomer Royal.*)

Percival Lowell. "Expedition for ascertaining the best Location for Observatories."

Percival Lowell. "A Standard Scale for Telescopic Observations."

The following gentlemen were elected Fellows of the Society:—

Francis Kennedy McClean, 1 Onslow Gardens, London, S.W.

Rev. Edwin Albert Phillips, B.A., Lecturer on Mathematics, Training College, Exeter.

The following Candidates were proposed for election as Fellows of the Society:—

Henry Bourget, D. ès Sc., Maître de Conférences à l'Université de Toulouse et Astronome-Adjoint à l'Observatoire de Toulouse (proposed by H. Deslandres).

Major John Cassells, V.D., J.P., 154 Queen's Drive, Crosshill, Glasgow (proposed by John Dansken).

Patrick Sinclair Hardie, M.A., B.Sc., Mathematical Master, 305 Onslow Drive, Dennistoun, Glasgow (proposed by J. McCarthy).

Richard Kerr, F.G.S., Lecturer on Astronomy, Experimental Physics, &c., 13 Ormiston Road, Greenwich, S.E. (proposed by E. W. Maunder).

THE BRITISH ASTRONOMICAL ASSOCIATION.

President: G. M. SEABROKE.

Secretaries: A. C. D. CROMMELIN and
J. G. PETRIE.

THE Thirteenth Annual General Meeting of the British Astronomical Association was held at Sion College, on Wednesday, October 29th, *Mr. G. M. Seabroke* (President) in the Chair.

The Minutes of the previous annual meeting having been read and confirmed, the names of four candidates for membership were read and suspended, and the election by the Council of five new Members was confirmed. A lengthy list of presents was also read.

The report of the Scrutineers of the ballot for the Council for the ensuing session gave the following appointments:—*President*: S. A. Saunder, M.A. *Vice-Presidents*: Mrs. E. Walter Maunder, Dr. Isaac Roberts, F.R.S., G. M. Seabroke, and W. H. Wesley. *Treasurer*: W. H. Maw. *Editor and Librarian*: F. W. Levander. *Secretaries*: A. C. D. Crommelin, B.A., and J. G. Petrie. *Other Members of Council*: Miss Gertrude Bacon, Rev. J. M. Bacon, G. F. Chambers, Dr. A. M. W. Downing, M.A., F.R.S., Alfred Fowler, W. T. Lynn, B.A., Capt. Wm. Noble, J.P., Mrs. Isaac Roberts, D. ès Sc., C. Thwaites, and C. T. Whitmell, M.A., B.Sc.

The Meeting unanimously confirmed the following appointments of Directors of Observing Sections, &c. which had been made by the Council:—*Editor*, F. W. Levander; *Sun*, Rev. A. L. Cortie; *Moon*, Walter Goodacre; *Mercury and Venus*, Henry MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Rev. T. E. R. Phillips, M.A.; *Saturn*, G. M. Seabroke; *Comets*, E. W. Maunder; *Meteors and Auroræ*, W. E. Besley; *Variable Stars*, Col. E. E. Markwick; *Double Stars*, G. M. Seabroke; *Star Colours*, E. J. Essam; *Photography*, R. Wilding; *Zodiacal Light*, H. O. Barnard.

The report of the Council on the work of the Twelfth Session, together with the accounts and balance-sheet, were unanimously adopted.

Votes of thanks to the retiring members of the Council, the Auditors, and the Scrutineers were also passed.

Mr. Seabroke then delivered the presidential address, in which, after reviewing the work of the past Session, he discussed the bearing of the most recent scientific discoveries and theories on the science of astronomy.

Dr. Downing, in proposing a cordial vote of thanks to Mr. Seabroke for his services as President during the past session, said they had just listened to a most admirable address. He was one of those who were strongly in favour of a continuance of the practice, which had been customary in connection with the Association, of the President delivering an address to the annual meeting Of course, it gave the President a good deal of trouble, and the address to which they had just listened must have entailed a considerable amount of research in its preparation; but he thought anyone who occupied the Chair at their annual meeting would not grudge the trouble an address involved in view of its value to the members, whether it be on the progress of the Association, or on the various points of astronomy which had come up for solution, and towards the solution of which the Members of the Association often contributed. Mr. Seabroke's address was not a whit inferior to those delivered on previous occasions. The retiring President during the term of his office had displayed a thorough grasp of the subjects discussed at the meetings, and the lucid manner in which he had treated them when called upon to speak has won the admiration of them all.

Mr. E. W. Maunder seconded the motion. It had, he said, been a great pleasure to him to have known Mr. Seabroke for so many years—ever since 1874, indeed—for one of the first things he had done on entering Greenwich Observatory was to go down to Rugby to see Mr. Seabroke, who had given him some lessons on the use of a particular form of spectroscope which Mr. Seabroke had devised. It had been a matter of great satisfaction to him to see Mr. Seabroke occupying the Chair of the Association for the last two years, and he heartily joined in Dr. Downing's appreciation of the great services that gentleman had rendered to the institution as President. He was extremely gratified to find that Mr. Seabroke had taken for the text of his address the great problems of the Stellar Universe. From the nature of the case, the papers ordinarily read before the Association dealt with special observations and particular points of work, not with wide reviews of the general progress of the science. It was, therefore, of special advantage to them to have so broad and able a summary of the present condition of those problems which were now attracting such attention, as they had just listened to from the President.

Mr. Seabroke, in acknowledging the vote, said the carrying out of his duties as President had been a work of love and pleasure, and he desired to thank all those connected with the Association for the able assistance and consideration he had received at their hands. As Mr. Maunder had stated, the subject of his address was coming up for solution, and he hoped that the members would do their best to help to solve it.

Mr. Maunder then presented the report of the Committee appointed towards the end of last Session to deal with the lantern-slide collection. He said that the old catalogue contained 1052 slides. Of these, 133 had been withdrawn, and 124 new slides had been added, making the present number 1043. During the recess, no fewer than 303 new slides, other than the 124 already mentioned, had been printed. Of these, 237 had been passed for acceptance, and of that number 84 had been bound up as slides, but were not yet numbered. The remaining 153 still required binding. When all were added to the Catalogue, it would contain 1270 slides. As the binding, numbering, and indexing of the 237 slides would take some time, the Committee proposed to reopen the lantern-slide collection for lending at once, and to prepare for temporary use rough slip proofs of the Catalogue in its present state, deferring the full publication of the Catalogue until it could be completed. In conclusion, Mr. Maunder moved the confirmation of the rules relating to the loan of the slides.

Mr. Newbegin seconded the motion, which was carried.

Mr. Maunder drew attention to a letter from Mr. Percival Lowell which had appeared in No. 3823 of the *Astronomische Nachrichten*, in which Mr. Lowell gave reasons for believing that the spoke-like markings which he had noticed on Venus had no objective existence, but were merely an optical effect. Experiments on artificial discs showed that similar markings appeared under corresponding conditions, though no such markings really existed on the discs.

Mr. Maunder added that the subject was of peculiar interest to him on account of its bearing on a matter to which he had been giving attention during the recess. In 1882, when Prof. Schiaparelli had brought out his system of double canals on Mars, he (the speaker) had drawn the attention of the Royal Astronomical Society to the circumstance that the canals appeared to be prolongations of irregularities of the "coast-lines" of Mars. Later, in 1894, he made a number of experiments on the limits of visibility for dark markings on a bright ground when markings existed, but were too minute to be fully defined. They were interpreted by the eye as straight lines if narrow, and as round dots if broad. As straight lines were more easily seen than dots, he had ventured to predict that the next discovery on Mars after the "canals" would be a system of "lakes," a prophecy promptly fulfilled by the discovery by Mr. Lowell of his system of "oases." He had recently received a letter from Mr. Lane (published in the November number of *Knowledge*), who had been experimenting in a slightly different direction. Mr. Lane had found that by submitting a drawing of Mars, but without canals, to be copied by sketchers at a distance, in two cases out of three the Schiaparellian canal system was sure to be seen. Acting on Mr. Lane's suggestion, he (the speaker) had tried the same experiment at the Royal Hospital School, Greenwich, with the hearty cooperation of Mr. J. E. Evans, the head master. Of 80 boys tested, one fifth saw and drew "canals," and it was clear that it was largely a matter of the distance away that the drawing was placed as to whether the canals were seen or not. But in his experience the canal-like markings did not arise without some provoking cause. There must be something—though it may not be a canal—to be a foundation for them.

Mr. Maunder, Director of the Cometary Section, said that two comets had been under observation during the recess. One of these, discovered by Mr. Perrine on September 1, was still under observation. The other case was a rather singular one, for the discoverer, Mr. John Grigg, of New Zealand. appeared to have been the only observer of the phenomenon. Mr. Grigg had reported that on July 22, 18^h 30^m G.M.T., he found a nebulous object, the position of which was roughly R.A. 11^h 35^m, Decl. +7°. On the following evening he again observed it about 24′ further south and 7^m eastward. Bad weather then intervened, but further observations were obtained on July 26 and August 1 and 2. The particulars had been communicated to continental and American observatories, but no other observer appeared to have seen it.

Mr. Whitmell reminded the Meeting of his paper of a few months ago, in which he had drawn attention to the fact that the surface of Saturn should be visible through the Cassini Division on July 17, 1902. He regretted to state that the replies had been few, and the results received were only meagre.

Mr. Crommelin thought that the failure to see the phenomenon might be due: (1) to the dusky penumbral fringes which bordered the shadow of the ring on the ball; (2) to the fact that the albedo of the ball (especially of the polar regions) was much lower than that of ring B; and (3) to optical illusion. The eye on seeing the greater part of an elliptical outline would tend to complete it.

A letter from *Mr. Allan B. Cobham*, of Mosman, N.S.W., was read, in which he drew attention to a new feature on Jupiter on the southern edge of the North Polar Cap at longitude 260°, first seen on July 31st last.

An interesting paper on " A Visit to Stonyhurst," by *Mr. Arthur Mee*, concluded the Meeting.

ROYAL METEOROLOGICAL SOCIETY.

THE first Meeting of this Society for the present Session was held on Wednesday evening, the 19th instant, at the Institution of Civil Engineers, *Mr. W. H. Dines, M.A.*, President, in the Chair.

Mr. F. Campbell Bayard read a paper " On English Climatology, 1881–1900," which was a discussion of the climatological data printed in the *Meteorological Record* from the 40 stations of the Royal Meteorological Society which have been continuous for the whole of the twenty years. The elements dealt with by the author are:—(1) temperature at 9 A.M.; (2) mean minimum temperature; (3) mean maximum temperature; (4) relative humidity; (5) amount of cloud; (6) rainfall; and (7) number of rainy days. The results form a valuable contribution to the climatology of the British Isles.

A paper by *Mr. C. V. Bellamy, M.Inst.C.E.*, on " The Rainfall of Dominica," was also read. This was in continuation of a former paper on the subject, and dealt with all the available rainfall data for the island of Dominica. From this it appears that the mean annual rainfall of the island is 110 inches. In the neighbouring island of Montserrat, a remarkably heavy rainfall occurred during the night of November 28–29, 1896, when as much as 20·13 inches fell in the space of six or eight hours.

Slow Meteors from Westerly Radiants.
Long-pathed Meteors.

The western sky usually exhibits few radiant-points, as the orbital motion of the Earth is directed from this quarter of the firmament and the meteors of such showers as are visible there must overtake us. Observers generally watch the eastern region, the radiants being more numerously clustered in that part. But during a long watch one or two meteors from a west, south-west, or north-west direction are pretty certain to attract special notice. They are usually bright, with very slow, long flights, and trains of yellow or red sparks. These westerly meteors are so impressive by their conspicuous and distinctive features, and by their unusual directions, that they cannot fail to incite the curiosity of the observer, though they are rarely present in sufficient numbers for him to discover their individual radiants, even by the combination of several nights' observations. He can, however, assign the various positions with tolerable safety if he is well acquainted with the minor showers and if the paths of the meteors have been very correctly recorded as regards their directions of flight. Mr. Corder once termed these particular objects "homeless wanderers," and there is something in their aspect suggestive of the title, since every watcher of meteors must have been struck with their apparent isolation as they make their occasional intrusion, like foreign bodies, among the ordinary swift shooting-stars furnished in the eastern sky. By far the most numerous class of fireballs consist of these slow-moving meteors, with radiants lying near the horizon somewhere in the west. The writer might select a large number of objects from amongst his observations in past years, but a few examples taken at random will suffice for the purpose. With regard to the comparative rareness or isolation of such meteors it may be mentioned that the Perseid of 1901 April 21 was the only one seen during 19¼ hours of observation between April 13 and 23 of the year named. The Scorpiid of 1887 July 19 was the solitary member of this shower recorded during 36 hours at about that date, while the β Ursid of 1886 August 4 was the only representative of that radiant seen in 27¾ hours.

Very slow, trained Meteors. W. Radiants.

Date.	G.M.T.		Mag.	Path.				Length of Path.	Duration.	Probable radiant.	
				From		To					
	h	m		α °	δ °	α °	δ °	°		α °	δ °
1887 Feb. 10	7	31	1	103	+58	165	+46½	38	7½ secs.	29	+ 8
1902 Mar. 23	7	25	<♀	188	+76	190	+35	41	4½ secs.	11	+39
1901 Apr. 21	10	41	3	202	+40	213½	+ 7	34	6 secs.	51	+43
1889 May 22	10	8	>1	287	+39	254¼	−15	62	16 secs.	63	+35
1887 July 19	11	43	1	298	+56	14	+65¼	36	V. slow.	250	−20
1886 Aug. 4	10	40	>♃	129	+60	83	+40	34	8 secs.	162	+59
1889 Sept. 25	8	5	1	332	− 7½	11	+ 8	42	13 secs.	250	−20
1884 Sept. 26	11	9	1	2	+51	51	+27	43	V. slow.	272	+22
1884 Sept. 27	11	40	3	326	+64	52	+48½	46	7 secs.	272	+22
1877 Oct. 16	16	30	4	204	+66	87	+37	66	9 secs.	230	+50

Meteors from radiants near the horizon usually have very long courses. They also travel slower than might be expected, and there is no doubt that upon entering the atmosphere they encounter resistance which very perceptibly reduces their previous velocity. In the case of meteors belonging to radiants near the Earth's apex and having little altitude, the evidence of retarded velocity is often marked, and I quote a few observed instances. The actual durations were only estimated, however, in regard to a few of the objects, but the description "slow" or "slowish" is noteworthy applied to meteors with theoretical velocities between about 35 and 45 miles per second.

Long-pathed, streaking Meteors. E. Radiants.

Date.	G.M.T.		Mag.	Path.				Length of Path.	Duration.	Probable radiant.	
				From		To					
	h	m		α °	δ °	α °	δ °	°		α °	δ °
1886 May 6	13	55	4	323	+30½	224	+79	62	Slow.	337	− 2½
1886 May 6	14	11	2	306½	+21	202	+31	90	Slow.	337	− 2½
1902 July 9	13	17½	1	8	+48½	239	+57	66	Slow; 3½ s.	22	+22
1880 Aug. 12	12	37	2	63	+34	165	+76	59	Slow.	57	+ 9
1887 Aug. 20	10	1	5	356	+67	297	+14	64	Slowish.	77	+56
1891 Aug. 28	10	20	3	84	+60	321	+46	64	V. slow.	99	+45
1900 Sept. 24	8	42	3	39	+55	357	+16	50	Slow.	132	+47
1877 Oct. 8	13	35	♃	46	+50	346	+32	47	Slow.	120	+16
1879 Nov. 12	10	17	2	98	+22	4	−15	98	Slow; 4 s.	148	+23
1876 Dec. 13	7	28	2	135	+66	334	+10	122	Slow.	150	+36

A very slow, long-pathed Aquarid was doubly observed on 1900 May 3, $13^h 58^m$, by Prof. A. S. Herschel, Slough, and Mr. J. H. Bridger, Farnborough, and it was found to have traversed a course of 155 miles with a velocity of 28 miles per second, whereas the parabolic speed would be 40 miles per second. Further observations of these long-flighted meteors afford the prospect of very interesting results in respect to peculiarities of motion operating during their luminous careers. Meteors from low radiants are often conspicuous spectacles, and are specially well suited for determining the speed. Some of the apparent changes in the aspects of meteors are very remarkable. When an easterly radiant has just risen its meteors will glide leisurely along arcs of the heavens amounting to 40°, 50°, or 60°, while a few hours later, when the radiant is high, the flights are short ones of about 10°, and exceedingly swift. The transformation is so great that the observer would scarcely admit them to be members of the same system but for their correspondence in direction and identity of radiant. Showers like the Librids, Scorpiids, Capricornids, Sagittarids, Aquarids, Piscid Australids, &c., commonly display meteors with very long flights to European observers, their radiants being never far above the horizon. Certain other streams, like the Cygnids and Cepheids of July and August, directed from lofty positions, give short, diving tracks, which often render it difficult to ascertain their radiants accurately.

Bishopston, Bristol,
1902, Sept. 20.

W. F. Denning.

P.S. Erratum.—In reference to the meteor of July 28, $11^h 7\frac{1}{2}^m$, mentioned in my letter, *Observatory*, November 1902, p. 394, the duration at Leicester was erroneously stated as 2 secs. instead of 1·1 sec. The correction requires an increase in the observed velocity from 23 to 30 miles per second, which is still, however, much slower than parabolic speed.

The Structure of the Universe.[*]

Dr. Osborne Reynolds, who is Professor of Engineering in the Owens College, Manchester, has been engaged for twenty years upon a research which promises to have a very remarkable effect upon our ideas as to the structure of the universe and the ultimate constitution of matter. Incidentally it attempts to explain the cause of gravitation and to tell us what electricity is, besides being full of side-lights on all physical problems. The outlines of this great discovery are traced in the little volume now before us, which contains Dr. Reynolds's notes for the Rede Lecture which he delivered at Cambridge last June. Unfortunately, they are presented in a fashion somewhat difficult for the ordinary reader, who will find his attention fully taxed in mastering the meaning of Dr. Reynolds's new theory as to the nature of matter. We do not here profess to criticise this theory, a task to which probably only half-a-dozen of the most eminent physicists in this country are equal. Whilst we wait to hear what Lord Kelvin and Sir Oliver Lodge, Professor J. J. Thomson and Professor Larmor, have to say on the matter, however, it seems worth while to present, in a more popular form than Dr. Reynolds has seen fit to adopt, some general notion of his theories.

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[*] This account of Dr. Osborne Reynolds's work, '*On an Inversion of Ideas as to the Structure of the Universe*' (Cambridge University Press, 1*s*. 6*d*.), is taken by permission from the *Spectator* of 1902, November 15.

From the time of Thales and Plato down to our own day, many attempts have been made to go behind the appearances of things and deal in thought with the material basis on which the physical universe must be supposed to rest. No coherent and completely satisfactory solution of the problem has hitherto been given. The difficulties of the research are so great—at least to beings of our finite powers—that many philosophers have been tempted to assume, with Berkeley, that there is no material basis below the appearances. Men of science have mostly contented themselves with working at the immediate and tangible elements in the problem, and have only handled theories of the ultimate structure of the material universe as convenient working hypotheses. The latest of these, which holds the field in so far that it is accepted on all hands as the most convenient way of explaining the phenomena of light and other forms of radiant energy—such as the Sun's rays and Marconi telegrams—is that of the so-called luminiferous ether, which is conceived as filling all space and serving as the medium by which the messages of light and heat and electricity are conveyed from place to place. The main idea, which originated two centuries ago in the mind of Huygens, and was elaborated by Thomas Young, is that of " space occupied by an incompressible elastic jelly yielding to tangential stress, having a density which is all but infinitely small." All the most common radiations can be explained as waves in this elastic jelly, propagated just like the ripples caused when a stone is flung into a pond. Mathematicians have been able to work out the conditions of wave-motion in such a medium, and thus to explain nearly all the observed phenomena of light and other forms of radiant energy on the hypothesis of its existence, prevading all space, both that which we call empty and that which is occupied by material bodies, through the network of whose atoms the ether is supposed to flow as freely as water under the Tower Bridge. The reason why such an ether has been invented is that the human mind is incapable of conceiving the possibility of " action at a distance," or an effect produced by one body on another without any physical connection. How far this inconceivability is due to the obsession of our own muscular sense is hotly disputed by metaphysicians, but few readers will deny that it exists. Newton put this, once for all, very clearly in a passage which remains the corner-stone of all meditations as to the structure of the universe:—"It is inconceivable, that inanimate brute Matter should, without the Mediation of something else, which is not material, operate upon, and affect other Matter without mutual Contact, as it must be, if Gravitation in the Sense of Epicurus be essential and inherent in it.......That Gravity should be innate, inherent and essential to Matter, so that one Body may act upon another at a Distance, through a Vacuum, without the Mediation of anything else, by and through which their Action and Force may be conveyed from one to another, is to me so great an Absurdity," says Newton, "that I believe no Man who has in philosophical Matters a competent Faculty of thinking, can ever fall into it." Inconceivability to man may not really be an infallible test of truth—"there are more things in heaven and earth," &c.—but it is the most cogent test that we can apply, though higher beings—living perhaps on that elastic body in the fourth dimension by which an eminent physicist has endeavoured to explain gravitation—may smile leniently at our impudence in making it so.

It is admitted, then, that all space must be filled with some medium which transmits the various influences which we know as radiant energy: for Newton's own theory that they might consist in the actual transmission of material particles has been completely disproved. The luminiferous ether has held its ground because it was adequate to explain most of the phenomena; but it has always been felt that our inability to assign any way in which it could possibly transmit the attraction of material bodies for one another, which we call gravitation, was a serious deficiency. Indeed no one has yet assigned a cause for gravitation—the fundamental property of all matter as we know it—which is even plausible. Such theories as Le Sage's bombardment of corpuscles, or Lord Kelvin's hypothesis of "sinks" and "sources" of matter, have been set aside, on the double ground that we have no proof of their existence, and that they presuppose such an incredible development of energy—gravitation being made a mere difference effect—as to be exceedingly improbable. But Dr. Osborne Reynolds tells us that his new theory of the universe accounts for gravitation on Newton's original hypothesis that it is the effect of a state of stress in the medium surrounding all bodies. That medium, as Dr. Reynolds believes that he has discovered it, is not the homogeneous infinitely rare jelly which is understood by those who have hitherto spoken of the ether, but a definite granular structure, comparable to an illimitable heap of shot or sand piled in a regular fashion so that each grain is at the same mean distance from each of its twelve neighbours. By a method which is intelligible only to the trained mathematician, Dr. Reynolds has been able to work out from his experiments a complete series of measures for the grains of this primordial substance which he believes to fill all space. The result is one of the most amazing propositions ever laid down by a serious physicist. We are told that the size of these grains is inconceivably small, though it has been definitely measured by mathematical analysis as being the seven-hundred-thousand-millionth part of the wave-length of violet light, a unit which is itself roughly equal to the sixty-thousandth part of an inch. In a normal condition these grains are in motion with a mean relative velocity of about one and one-third feet per second, though the mean path of each grain is restricted by its neighbours to the four-hundred-thousand-millionth part of its own diameter. From this it follows that the mean density of such a medium must be ten thousand times that of water, or four hundred and eighty times greater than that of the densest matter known on the earth; whilst it must be normally in a state of stress such that its mean pressure is seven hundred and fifty thousand tons on the square inch, or three thousand times greater than the strongest material known to us can bear. If, now, the reader will try to conceive the real meaning of Dr. Reynolds's assertion that the whole of space—which we call empty—is filled by this extraordinary medium—recalling Wordsworth's description of

> "The heavy and the weary weight
> Of all this unintelligible world"

—he will have some idea of the epoch-making nature of this discovery.

Dr. Reynolds goes on to state—the detailed proof is reserved, and, as we have said, only a very few men can be competent to criticise it—that the assumption of such a medium affords "a complete, quantitative, purely mechanical explanation of the cause of gravitation." It is enough to say here that this cause is found in the tendency of any system of grains arranged in the hypothetical manner to expand under pressure, a tendency most ingeniously illustrated by the behaviour of indiarubber bags filled with sand and water, one of which the author proudly calls "the first experimental model universe." Another almost equally striking consequence is that the hypothesis gives us "a purely mechanical explanation of electricity." What, then, one asks, is matter? The answer to this question also is ready, and not wholly novel to those who are familiar with Lord Kelvin's beautiful theory of vortex atoms,—matter, as we know it, is simply a mode of motion in the assumed granular medium. "We are all waves," says Dr. Reynolds, recalling Montaigne's description of man as "divers et ondoyant." We have given but a bald account of Dr. Reynolds's fascinating, and perhaps epoch-making, theories, but it is with the greatest interest that we await the opinion on them of the few men of science whose judgment will carry weight.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Invention of the Telescope.

GENTLEMEN,—

The appearance of Mr. Maunder's interesting and suggestive little work, 'Astronomy without a Telescope,' naturally carries us back in thought to the time when all astronomy was without a telescope, and to the disputes about the priority in its invention. An account of the history of the matter is given in Pingré's 'Annales célestes du Dix-septième Siècle,' recently published under the auspices of the Académie des Sciences, and edited by M. G. Bigourdan. Although this agrees in all essential points with that in Sir David Gill's article on "The Telescope" in the ninth edition of the 'Encyclopædia Britannica,' yet several of Pingré's remarks are of interest now. It is well known that Descartes attributed the invention to Jacob Métius. And indeed it is quite possible that he may have independently discovered the means of magnifying distant objects by a combination of two lenses, but whether he or Lipperhey was the first appears doubtful; both applied to the States-General of Holland within a fortnight of each other for the exclusive right to make instruments for this purpose. Lipperhey's application was the first, but that of Métius seems to have deterred the States-General (whilst purchasing two instruments made by the former) from granting any exclusive rights. As to Jonsius, to whom Pierre Borelli attributed the invention in 1590, we shall all agree with Pingré that, even if it be true, "nous ne devons aucune reconnaissance à Jonsius, qui durant 19 années imitant, s'il est permis de le dire, le chien du jardinier [the dog in the manger, as we call it in England], n'a retiré lui-même aucune utilité de sa découverte, et nous a privé des fruits qu'elle auroit produits, si elle fut parvenue à la connaissance des Tycho, des Mœstlin, des Képler, &c."

But there is no room to doubt that the idea had occurred to Giambattista Porta, of Naples, long before, being set forth in his treatise, 'De Magia Naturali,' which appeared in 1560. In this he speaks of a means of combining convex and concave glasses so as to make distant objects appear nearer; but there, says Sir David Gill, "his interest in the matter appears to have ceased, and he was unable either to appreciate the importance of his discovery or to describe the means by which the object was attained." Kepler, however, thought that Porta wrote purposely in an unintelligible manner in order to keep his secret; yet, as Pingré remarks, his words "nous paroissent cependant très-claires." However, one fact is incontrovertible, that Porta did not bring his discovery into any practical use, and though Métius applied his combination of two lenses to the magnification of distant terrestrial objects, it seems to have been left to Galileo to apply it first to celestial observations. The claim of Mayr, or Marius, to have anticipated him in the discovery of Jupiter's satellites was not made for some time afterwards, and is not generally accepted by astronomers. Kepler, I believe, was the first to use two convex lenses.

It is, perhaps, hardly worth while to refer again to the claim to the invention of the telescope which has been set up on behalf of Roger Bacon. It is evident from his own writings that he noticed the magnifying and diminishing powers of convex and concave lenses; but there is a very great step from that to combining two lenses so as to bring distant objects near, which is the function of a telescope as we use that word (in accordance with its literal meaning), though the French reserve it for a reflecting telescope only. The very extravagance of the claim put forth on the supposed authority of Bacon long after his own time is sufficient to refute it. For we are told that he said the invention was in reality made centuries earlier, and that Julius Cæsar, before invading Britain, surveyed its coasts and buildings from the Gallic side of the Channel by the aid of glasses. Had this been possible, very different indeed would have been the account of his first voyage, and the previous sending of Volusenus to acquire information would have been quite unnecessary. If Bacon did really say anything which was perverted into this, it can only have been that, meditating on what he had noticed of the effect of lenses, he amused himself by imaginary speculations on the extent to which this might conceivably be carried. Yours faithfully,

Blackheath, 1902, Nov. 3. W. T. LYNN.

The Red Spot Region of Jupiter.[*]

GENTLEMEN,—

I am afraid that Herr Leo Brenner has been endeavouring to raise a difficulty where none existed. In a vain effort to identify particular sections of an extensive disturbance on Jupiter his own observations have clearly shown how easily anyone may fall into error in tracing the details of a marking undergoing rapid changes and material developments. "The third pyramid spot" which he saw at the following end of the great red spot on 1902, Aug. 13, he suddenly found at the preceding side of that object twelve days later (Aug. 25). It was, in fact (as stated in my letter in the *Observatory* for October), far west of the position he had previously assigned to it; but he gets over the difficulty readily enough on the assumption that a new object, "the first pyramid spot," must have broken out in the interval, while the third "pyramid" had been lagging behind! This is all very well, but it shows the doubts and complications in which your correspondent has become involved, and the utter insecurity of forming definite conclusions on data of this kind.

Herr Brenner's statement that Mr. Phillips and myself imputed a diurnal motion of 7° to one and the same spot is ridiculous, and it is curious how he could have misapprehended the simple facts. The longitude of 80° on June 19, and of 19° on June 28, referred to the *following* and *preceding* sides, respectively, of a marking covering a very large extent of longitude! It would be very unfair were we to charge Herr Brenner with attributing a motion to the red-spot-hollow of 36° in twelve days because he saw its *following* end in 62° on Aug. 13 and its *preceding* end in 26° on Aug. 25. Yet this is precisely the argument which your correspondent has employed against Mr. Phillips and myself in regard to the south tropical marking on Jupiter. Herr Brenner apparently quite fails to understand that we have been considering the latter spot *as a whole*. If he can only be induced to adopt the same plan his very dubious identifications may be avoided, and there will be no necessity to assume that any of the "pyramids" are lagging behind or that new ones are suddenly springing into existence.

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[*] [We have received a letter to the same effect as this from Mr. T. E. R. Phillips.—EDS.]

Herr Brenner remarks that when he first measured the marking in 1901, August, it consisted of but one "pyramid"; but it is certain that it was formed by three projections from the southern equatorial belt some time before (see Dr. Kibbler's drawings in *B. A. A. Journal* for Jan. 1902, vol. xii. p. 124).

Yours sincerely,
W. F. Denning.

P.S.—During recent months one of the most striking features of Jupiter has been the remarkable brilliancy of the equatorial zone. There used to be a number of dark wisps and dusky details here with an occasional narrow belt, but all these features appear to have been practically obliterated in a flow of highly luminous material covering the whole space between the two equatorial belts. Another noteworthy fact, in the general aspect of the planet, is that the N. hemisphere is very dusky and relatively much darker than the S. hemisphere. With a low power the intensely bright equator and the dull N. hemisphere are well brought out even on bad nights—and really good ones have been like angels' visits in recent months.

Bishopston, Bristol,
1902, Nov. 5.

Astronomy at the British Association.

Gentlemen,—

In the issue of *Nature* for October 23 there is a short note on "Astronomy and Cosmical Physics at the British Association," signed with the initials "A. R. H." In your issue of *The Observatory* for November this note is reproduced as an article, attributed to the conjectural owner of the above initials, and in a footnote to the title it is stated that the note was communicated to *Nature* by the Secretary of the Subsection.

The concluding sentence of the note has been omitted in the reprint. It was:—"Other papers read at the meetings of the Subsection have already been mentioned in *Nature* in the notes of mathematical and physical papers." In the issue of *Nature* for October 16, in an account of the mathematical and physical papers read before Section A, mention was made of papers read in the Subsection of Astronomy and Cosmical Physics by Mr. J. Milne, Mr. W. N. Shaw, Mr. W. E. Wilson, Dr. Isaac Roberts, and Father Cortie; of the discussion on the nebula surrounding Nova Persei; and of the Report of the Kites Committee. These are the papers referred to in the sentence quoted above. By omitting this part of the note of October 23, and attributing the rest of it explicitly to the "Secretary of the Subsection"—technically there is no such person; he is just one of the six Secretaries of Section A—you have, it seems to me, made it appear that a slight has been cast upon the authors of the papers in question.

I think that it is due to those gentlemen that you should make it clear, by publishing this letter, that this omission by *The Observatory* of all reference to their papers is an oversight of your own, and is not due to the Secretary of Section A, whom you have, conspicuously but without his knowledge, made apparently responsible for your account.

I am, Gentlemen,
Yours very faithfully,
1902, Nov. 5. The Writer of the Note in *Nature*.

The Distortion of Object-Glasses.

Gentlemen,—

In your issue of June last appears an article by your correspondent "From an Oxford Note-Book" (hereinafter designated Ox. for brevity), which calls for some notice on my part. Ox. does me the honour of approving my method of testing astronomical photographic objectives for distortion of the field; but considers that I have reached a false conclusion in my application of the method to the Cape of Good Hope telescope.

I may say at once that I wish Ox. were right. If my method were ingenious (as Ox. says), and if, as I believe, it is perhaps the best method for testing lenses, it would have given me the greatest pleasure to arrive at strong positive results. I wish I could have found plenty of optical distortion; what I did find I now repeat in my own words, placed by the side of the same thing, as translated into Oxford Notes.

Jacoby's statement, as made by him.	*As given by Ox.*
If there is any meridian section of the lens capable of causing optical distortion, it must show itself by a clustering of the dots above or below the horizontal zero-line (in my diagram); since no decided clustering appears, we may conclude that there is no optical distortion large enough to be detected.	. . .; and, as a result, Mr. Jacoby announces that there is no optical distortion of the kind considered.

It appears to me that the right-hand column above makes me deny the existence of distortion more vigorously than it should; my word *decided* was intended to mean that I was aware of certain peculiarities in the arrangement of residuals, but did not consider them sufficiently large and well-determined to warrant the preparation of distortion-correction tables applicable in general to C.G.H. plates.

But there would be little scientific interest in stating the above, were it not for the opportunity it gives me of emphasizing again the bearing of my investigations on the whole subject of the reduction of astronomical photographs.

I have used the absence of decided distortion as a proof that four constants are sufficient for the reduction of photographs; but I do not agree with Ox. in his opinion that the presence of distortion helps to prove the advisability of a six-constant solution. If distortion exists, I should advise the formation of an empirical table of corrections founded directly on the observations. Ox. has gathered my distortion-residuals into three-hour means for position-angles only, omitting the distances. His figures are set down in the accompanying table, together with corresponding ones for the distances.

Table of Distortion-Residuals.

Position-angle.	Mean. θ.	Residual.		Position-angle.	Mean. θ.	Residual.	
		$r\Delta\theta$.	Δr.			$r\Delta\theta$.	Δr.
h h	h	"	"	h h	h	"	"
23 to 1	$0\frac{1}{2}$	+·07	−·18	11 to 13	$12\frac{1}{2}$	·00	−·06
0 „ 2	$1\frac{1}{2}$	+·11	−·12	12 „ 14	$13\frac{1}{2}$	+·03	−·12
1 „ 3	$2\frac{1}{2}$	+·12	−·08	13 „ 15	$14\frac{1}{2}$	+·07	−·10
2 „ 4	$3\frac{1}{2}$	+·07	+·05	14 „ 16	$15\frac{1}{2}$	+·08	−·08
3 „ 5	$4\frac{1}{2}$	+·03	+·01	15 „ 17	$16\frac{1}{2}$	+·08	·00
4 „ 6	$5\frac{1}{2}$	+·04	−·04	16 „ 18	$17\frac{1}{2}$	+·07	+·05
5 „ 7	$6\frac{1}{2}$	−·04	−·01	17 „ 19	$18\frac{1}{2}$	·00	+·18
6 „ 8	$7\frac{1}{2}$	−·09	+·04	18 „ 20	$19\frac{1}{2}$	−·04	+·19
7 „ 9	$8\frac{1}{2}$	−·11	+·08	19 „ 21	$20\frac{1}{2}$	−·08	+·14
8 „ 10	$9\frac{1}{2}$	−·07	+·08	20 „ 22	$21\frac{1}{2}$	−·12	+·01
9 „ 11	$10\frac{1}{2}$	−·05	+·06	21 „ 23	$22\frac{1}{2}$	−·09	−·08
10 „ 12	$11\frac{1}{2}$	−·04	+·01	22 „ 24	$23\frac{1}{2}$	−·07	−·15

From the periodicity in the column of angle-residuals, headed $r\Delta\theta$, Ox. argues the presence of distortion and the desirability of a six-constant solution. He gives the following:—Putting the correction required by y in my four-constant solution equal to $2\mu y$, he finds for the corrections required by the angles θ and the distances r:

$$r\Delta\theta = \mu r \sin 2\theta, \qquad \Delta r = \mu r(1 - \cos 2\theta).$$

Having computed values of $r\Delta\theta$ only (see Table), he determines

μ, assuming an average value of $r = 2000''$. But by division the above equations give:

$$\frac{r\Delta\theta}{\Delta r} = \cot\theta.$$

This is a relation that should hold good at any point of the table between the angle- and distance-residuals. It does not: and this constitutes a proof that even if my residuals exhibit distortion, it is not of such a character as would be eliminated by a six-constant solution.

It remains, therefore, to inquire whether it can be dealt with by an empirical table of corrections. As a first step I have re-arranged the residuals, taking separate means for the stars distant more and less than 2000″ from the plate-centre.

Residuals according to Distance.

Mean. θ.	$r > 2000''$.		$r < 2000''$		Greater − Less.	
	$r\Delta\theta$.	Δr.	$r\Delta\theta$.	Δr.	$r\Delta\theta$.	Δr.
h	″	″	″	″	″	″
0½	+·03	−·19	+·12	−·16	−·09	−·03
1½	+·03	−·12	+·17	−·12	−·14	·00
2½	+·11	+·03	+·10	−·09	+·01	+·12
3½	+·09	+·08	+·05	−·02	+·04	+·10
4½	+·05	·00	+·03	+·03	+·02	−·03
5½	·00	−·08	+·12	+·02	−·12	−·10
6½	−·10	−·02	+·10	+·02	−·20	−·04
7½	−·13	+·06	+·04	·00	−·17	+·06
8½	−·12	+·10	−·08	·00	−·04	+·10
9½	−·08	+·09	−·06	+·06	−·02	+·03
10½	−·04	+·06	−·08	+·06	+·04	·00
11½	−·03	−·02	−·08	+·09	+·05	−·11
12½	+·04	−·08	−·10	−·01	+·14	−·07
13½	+·08	−·12	−·13	−·13	+·21	+·01
14½	+·14	−·10	−·04	−·12	+·18	+·02
15½	+·11	−·08	+·05	−·09	+·06	+·01
16½	+·11	·00	+·04	·00	+·07	·00
17½	+·11	+·05	−·02	+·04	+·13	+·01
18½	+·04	+·19	−·06	+·15	+·10	+·04
19½	−·02	+·22	−·04	+·17	+·02	+·05
20½	−·09	+·16	−·04	+·11	−·05	+·05
21½	−·13	−·02	−·10	+·06	−·03	−·08
22½	−·11	−·13	−·07	−·01	−·04	−·12
23½	−·10	−·20	·00	−·05	−·10	−·15

The differences "Greater *minus* Less" are large and periodic; they show that any empirical table must be complicated, and must have both distance and angle for arguments. They supply an additional proof that the substitution of six constants instead of four is insufficient to remove the supposed distortion.

It would occupy too much space to reproduce here certain further calculations I have made to ascertain whether the supposed distortions are repeated on the several plates. My conclusion is that they are not so repeated accurately enough to justify an empirical table of corrections for use on future plates. In spite of Ox.'s opinion to the contrary, I do not think I have succeeded in getting sufficient distortion material to correct C.G.H. plates; and I repeat that even if they are to be so corrected, it must be done by an empirical table of corrections, and cannot be done by simply using a six-constant solution.

Columbia University, New York,
1902, Oct. 28.

Yours faithfully,
HAROLD JACOBY.

[Mr. Jacoby started out to investigate whether one particular form of distortion existed, and concluded that there was none "large enough to be detected." My note pointed out evidence controverting this proposition: but certainly did not deny the possible existence of other forms of distortion. In writing a brief note only a small part of a large question can be dealt with, and other matters must be left for some one with leisure to go into the whole matter, or perhaps to the common-sense of the reader. One point which it was hoped would not be misunderstood was that the term $\Delta r = \mu r(1 - \cos 2\theta)$, being not wholly periodic, will not correspond to residuals as it stands: it has been partly absorbed in the scale-value adopted for the plate. Hence Mr. Jacoby's test $\frac{r\Delta\theta}{\Delta r} = \cot\theta$ is quite inadmissible. When he fails to interpret his own evidence, and suggests inadmissible tests of this kind, I would ask him whether he can expect us to be much impressed with his "advice" that an empirical table of corrections should be formed and applied? I think we should prefer to wait until the evidence has been thoroughly and properly examined.—"Ox."]

OBSERVATORIES.

MELBOURNE.—The Report of the Board of Visitors of this Observatory for the year ending 1902 March 31 reiterates the necessity for the appointment of a Chief Assistant. The Visitors say that Mr. Baracchi is eminently qualified to undertake astronomical research, but has not the opportunity to pursue this because he has no assistant able to do the supervision which now falls to his share, and it is added that though the present staff do their observing and computing well, there is no one among them competent to take charge of an Observatory even for 24 hours, which seems rather severe on these gentlemen, of whom, however, we have no personal knowledge. This obviously raises again the question whether it is the duty of the Director of an Observatory to make astronomical research, or whether he should content himself with administration, on which we will not here express an opinion.

As to the staff, it may be seen that besides the permanent members, who are, speaking generally, observers and photographers, there is a fairly large temporary staff. Five young ladies are engaged in the astrographic measuring bureau, five more in reducing arrears of meteorological records, and four men are reducing the 30-year photographic records of terrestrial magnetism, which are very valuable pieces of work in three different branches of science. The astronomy now being done at Melbourne is entirely in connection with the astrographic catalogue. With the Meridian Circle the fundamental stars for reduction of the plates are being observed, and of these a little over a thousand observations were made during the year, which number does not seem excessive, and there is no note that the weather was especially unfavourable during the year. Remembering a remark in an earlier number*, it is interesting to read a note in this section to the effect that a certain unsteadiness of instrumental errors has been traced to imperfect drainage and has been remedied. Three hundred and twenty plates, including Chart, Catalogue, and others, have been taken and passed as satisfactory, and in the Report, which is signed jointly by Mr. Russell of the Sydney Observatory and Mr. Baracchi of Melbourne, it is stated that 158 plates containing 76,456 stars were measured in the direct and reversed positions during the year, which completes 267 plates of the whole number (2549) allotted to the two Observatories. The Repsold measuring micrometer, which is in process of making, has not yet reached Melbourne, and the measuring machine devised by Mr. H. C. Russell is not yet completed, so that these plates have been measured with filar micrometers, which are not expected to do more than a third or one fourth of the work that can be done in the same time with the Repsold.

There is a considerable amount of other work of different kinds which falls to Mr. Baracchi — tide-registration, seismography, testing chronometers and other instruments for the public; and he is shortly to be charged with work in connection with the bureau of weights and measures, so that he will have his hands full.

PUBLICATIONS.

Astronomy without a Telescope †. — The object of this excellent and compactly arranged book is succinctly set froth in its subtitle— it is designed to serve as "a guide to the constellations, and an introduction to the study of the Heavens with the unassisted sight." Keeping this object rigidly in view, the author proceeds in Section 1 to set before the student an elaborate course of instruction relating (1) to the ancient grouping of the stars into constellations; (2) to the origin and meaning of the names of the constellations and principal stars; and (3) to their relative positions and duties as time-indicators, so that, with our great progenitor, the reader may realize that

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* See October, p. 373.

† 'Astronomy without a Telescope.' By E. Walter Maunder, F.R.A.S. 1902. London: *Knowledge* Office, 326 High Holborn. Price 5s.

> . . . . . . "Heaven
> Is as the book of God before thee set,
> Wherein to read His wonderous works, and learn
> His seasons, hours, or days, or months, or years."

The enquiring student having mastered this mass of information presented in a clear and attractive form, helped by means of numerous diagrams and charts, is next invited to proceed on a personally-conducted tour with the planets in their courses through the heavens. This comprises Section 2, and is invested with a graphic power characteristic of the facile pen which has already given to the world so many picturesque works.

Finally, in Section 3, the various observations which fitliest fall to the province of the observer unprovided with instruments are enumerated. These comprise observations of meteors, of the zodiacal light, of auroræ, of the Galaxy, of new stars and comets, of eclipses, &c., all of which are dealt with in a lucid and attractive manner. It appears invidious to point out a flaw where so much is excellent, but it certainly seems that in the chapter on meteors so much credit has been given to Mr. Denning that the author has scarcely done justice to the indefatigable labours of earlier workers in this field of research. Before Mr. Denning began to observe in 1870, full and complete lists of meteor-radiants had been drawn up by Greg and Alex. Herschel, based upon observations made in England in the years 1848 to 1870, by Schiaparelli and Zezioli from observations in Italy from 1867 to 1870, and by Heis from observations made in Germany from 1847 to 1866, and these lists, printed in the Report of the British Association for 1872, probably served as foundation for Mr. Denning's lists of principal radiants as given in this book and in other places. This omission, however, does not detract from the merit of the book, which may confidently be recommended to the study of the intelligent observer of nature in search of a hobby. W. C. N.

NOTES.

COMET NOTES.—The following elements of Comet *b* 1902 (Perrine) have been deduced by Elis Strömgren from observations on September 1, 20, and October 8; they may be relied on as extremely near the truth:—

T	1902 Nov. 23·88925 Berlin M.T.	
ω	152° 57′ 28″·2	1902·0.
☊	49 21 7·5	
i	156 21 9·8	
log *q*	9·603246	

Ephemeris for Berlin midnight.

Date.	R.A. h m s	S. Dec. ° ′	Brightness.
1902 Dec. 9......	15 22 48	25 3	9·3
,, 13......	15 10 46	26 54	7·6
,, 17......	14 58 32	28 50	6·9
,, 21......	14 45 33	30 54	6·3
,, 25......	14 31 4	33 8	5·9
,, 29......	14 14 15	35 31	5·7
1903 Jan. 2......	13 54 0	38 4	5·6
,, 6......	13 28 55	40 40	5·5
,, 10......	12 57 24	43 11	5·6
,, 14......	12 18 9	45 15	5·6
,, 18......	11 31 0	46 23	5·4
,, 22......	10 38 34	46 2	5·2
,, 26......	9 46 17	43 58	4·7
,, 30......	8 59 34	40 24	4·1
Feb. 3......	8 21 13	35 58	3·4
,, 7......	7 51 16	31 16	2·7
,, 11......	7 28 23	26 47	2·1
,, 15......	7 11 2	22 42	1·6
,, 19......	6 57 52	19 6	1·3
,, 23......	6 47 54	15 58	1·0
,, 27......	6 40 22	13 16	0·8
Mar. 3......	6 34 46	10 56	0·6
,, 7......	6 30 53	8 54	0·4
,, 11......	6 28 47	7 2	0·3
,, 15......	6 28 49	5 13	0·2

The brightness on September 1 is taken as unity; the comet was then brighter than the 9th magnitude, so that there is little doubt that it will be again telescopically visible in these latitudes after the middle of February. It will then be in Canis Major, passing 3° north of Sirius on February 27. It will thus almost complete the tour of the heavens during its period of visibility, and its orbit elements should be determinable with very great accuracy. Up to the present there is no decided evidence of deviation from a parabolic orbit, so that the period must probably be reckoned at least by centuries.

It has been pointed out by Mr. Seagrave that the comet will pass within two millions of miles of Mercury at the end of

November: the perturbations produced by this planet are, however, very small; they are investigated by Herr Strömgren in *Ast. Nach.* 3827. The total resulting shift in the comet's place on March 9 next is only about 70 miles—about ¼ of a second of arc as seen from the Earth—which, in an ill-defined object like a comet, is practically negligible.

Ast. Journ. 525 contains definite elements of Comet 1898 IX., by Henry A. Peck. This comet was discovered by Perrine, 1898, September 12, and independently by Chofardet two days later. The last observation was made on October 10, and consequently the orbit cannot be very precisely determined; the following is the most probable parabola:—

			Probable error.
T	1898 Oct. 20·57681 Berlin M.T.		·00026
ω	162° 21′ 29″	1898·0.	13″
☊	34 53 12		5
i	28 50 58		1
$\log q$	9·623635		·000030.

The observations indicate a hyperbolic orbit with eccentricity 1·00012; but the parabolic orbit is considered almost equally probable.

Ast. Nach: 3827 contains definitive elements of Comet 1881 II. (Swift) by Dr. Kreutz. The comet was only observed for 10 days (May 2–11), and consequently the orbit is somewhat doubtful. Previous computers had deduced hyperbolic elements, but Dr. Kreutz by rejecting the O'Gyalla observations is able to represent the 5 normal places deduced from the remaining ones with no greater error than 2″·7. He accordingly adopts the following parabola as the definitive orbit:—

T	1881 May 20·478155 Berlin M.T.	
ω	173° 47′ 39″	1881·0.
☊	126 26 52	
i	77 56 39	
$\log q$	9·771653	

Ast. Nach. 3829 contains definitive elements of Comet 1863 I. (Bruhns) by A. von Flotow. He deduces the following hyperbola:—

T	1863 Feb. 3·52578 Berlin M.T.	
π	191° 22′ 29″	1863·0.
☊	116 55 34	
i	85 21 56	
$\log q$	9·900244	
e	1·000071	

He also gives parabolic elements which he considers equally probable; they are practically the same as the above.

Mr. John Grigg has deduced the following approximate elements of the comet discovered by him on July 23 last (*Ast. Nach.* 3828):—

T	1902 June 20·33 G.M.T.
ω	301° 46′
☊	217 31
i	16 43
log *q*	9·7662

As his observations are rough, the uncertainty of these elements is considerable.

Ast. Nach. 3828 contains the following search ephemeris of Comet Tempel-Swift by J. Bossert. The comet passes perihelion towards the end of January next, and makes its nearest approach to the Earth (about 1·4 units) about the same time.

Ephemeris for Paris midnight.

	R.A.			S. Dec.			R.A.			Dec.	
	h	m	s	°	′		h	m	s	°	′
1902 Dec. 5......	21	4	57	11	56	1902 Dec. 30......	22	20	51	5	21 S.
10......	21	19	0	10	50	1903 Jan. 4......	22	37	37	3	44
15......	21	33	38	9	37	9......	22	54	54	2	3
20......	21	48	51	8	17	14......	23	12	38	0	17 S.
25......	22	4	35	6	52	19......	23	30	53	1	33 N.

A. C. D. C.

Minor Planet Notes.—The following planets have been discovered at Heidelberg:—

	Date.	Discoverer.	Magnitude.
KB	1902 Oct. 25	Wolf.	12
KC	"	"	13
KD	"	"	11
KF	"	"	13
KG	"	"	12
KH	"	"	12
KJ	Nov. 4	Dugan.	12
KK	" 4	"	12
KQ	" 21	Wolf.	12
KT	" 22	"	13

Planets 477, 478, discovered by Dr. Carnera last year, have been named Italia and Tergeste.

A planet named KE proved to be identical with (19) Fortuna.

A. C. D. C.

Co-operation in observing Velocities in Line of Sight.—It is generally agreed that the astronomy of the future must deal with cosmical problems, and to this end must provide proper motions, parallaxes, and radial velocities of stars wholesale. For the first of these, much material exists, and more is accumulating; there is not so much information of the second kind available, and as to the third class, Prof. E. B. Frost points out, in the *Astrophysical Journal* for October, that it is scarcely possible to find a dozen stars whose radial velocities have been well determined at three observatories. To remedy this he has asked workers in this branch of astronomy to combine in observing stars from a specially drawn-up list, and a small beginning of a working cataolgue has been formed—each star in which will have its radial velocity determined three times per annum by each co-operating observer.

"STELLAR COLOUR AND ITS EFFECT UPON DETERMINATIONS OF PARALLAX."—Under this title Prof. Comstock has an interesting paper in the *Astronomische Nachrichten*, No. 3821. He sets out by recalling Prof. Seeliger's paper in No. 3795, where the latter applies to Hartwig's determination of the parallax of Nova Persei, $\pi = -0''{\cdot}16$, the hypothesis that this negative value is due to a difference of colour between the Nova and its comparison stars, which produces a difference in the mean refrangibility of their light and therefore a difference in their atmospheric refractions. Assuming the true value of the parallax to be not sensibly different from zero, he finds from a solution of the six observation equations a range of 80 $\mu\mu$ in the mean wave-lengths of the three stars under consideration.

Prof. Comstock considers this a case quite similar to the light-equation in observed transits of stars of different magnitudes. After mentioning previous discussions by Gill and Chase, who found no appreciable effect due to colour, he states that his own eye is sensible, to an amount of variation in the mean wave-lengths, from the whitest to reddest stars, not exceeding 20 $\mu\mu$. Since the publication of Seeliger's paper he has made further investigations, employing a series of wooden gratings in front of a 40-cm. objective, and measuring with a filar micrometer the angular distance between corresponding spectra on opposite sides of the central image of a star. Three gratings, of intervals 6·4, 19·0, and 39·9 mm., were used, so that the lower orders of spectra were in general practically stellar discs, and their distances could therefore be measured precisely as if they were wide double stars varying from 5″ to 40″ with a mean of 30″.

Prof. Comstock then gives a table of "effective wave-lengths," which, he says, show a dependence on the colour of the star. The effect is small, and agrees in the main with the observations of Gill and Chase. The entire range of effective wave-length from the whitest stars, such as Vega and Altair, to that of the ruddiest ones, *e. g.* Aldebaran, Antares, and even Mars, does not much exceed 20 $\mu\mu$, a result only one-fourth that obtained by

Prof. Seeliger for the Nova and stars. The following "effective wave-lengths" are taken from the table:—

25 Cygni	553	α Serpentis ..	567
Vega	556	δ Ophiuchi ..	569
Altair	559	Capella	572
Spica	560	Antares	575
Arcturus	565	Mars	586

Bidston Observatory.—From paragraphs in the Liverpool newspapers it appears that the disestablishment of this observatory is in contemplation, for among the provisions of a Bill which is being brought before Parliament by the Mersey Docks and Harbour Board is one to relieve them of all necessity to maintain the observatory and to authorize the Board to sell the building and instruments. There seems to be some local feeling against this course, as would naturally be expected, and the suggestion is made from some quarters that the observatory should be handed over to the University soon to be established in the city.

The Variability of α Orionis.—This star (Betelgeuse) is given in Chandler's Third Catalogue of Variable Stars, magnitude at maximum 1, at minimum 1·4, irregularly periodic. It was noted by Mr. Packer, of Birmingham, at the end of October, that the star was then unusually bright. The fact has been confirmed by Mr. J. E. Gore, who, writing to the *English Mechanic* on October 24, quoted from his note-book that on October 16 the star was as bright as Capella (0·2), and nearly as bright as Arcturus (0·0) on October 17.

Erratum.—Last month's number, p. 411, line 7, *for* "maximum" *read* "minimum."

Prof. Karlinski has resigned his post as Director of the Cracow Observatory, and is succeeded by M. Maurice P. Rudzki.

Sir Wm. Huggins is President of the Royal Society for the ensuing year, and Prof. H. H. Turner is a Member of the Council.

The next Meeting of the Royal Astronomical Society will be on Friday, December 12; of the British Astronomical Association on Wednesday, December 31.

FROM AN OXFORD NOTE-BOOK.

In these days when the Education Bill brings specially to our minds the question of the endowment of schools of all kinds, the following notes may be of interest.

In Rigaud's 'Memoirs of Bradley,' which form the introduction of 'Bradley's Miscellaneous Works,' occurs the following passage:—

> He was educated in the grammar school at Northleach, under the tuition of Messrs. Iles and Brice. Hugh Westwood, Esq., the founder, placed it under the superintendence of Queen's College, Oxford, and from the registers of that society, it appears that Iles (not Egles as De Fouchy has written it) was master at this time: Brice was his assistant. Bradley continued under their care till he went to Oxford, and those who are interested in the credit of the school still cherish the tradition of his having been educated at it.

These words were written about 1831. Curiosity provoked me to make enquiry of a Fellow of Queen's College concerning the position of the school at the present day; and he kindly replied as follows:—

> Until about 1870 the College was Visitor of Northleach School, and our Provost appointed the Head and Second Masters. Under the new scheme, about 1870, the College appoints two Governors. When the change took place, two of the Masters—the Head and Second, I believe—were pensioned off. Now the emolument is the great tithe of Chedworth, and owing to the fall of tithe the whole income of the foundation is swallowed up by the pensions, and some four years ago the school was closed.

A further enquiry addressed to the late Head Master, Mr. F. Godwin, who still lives in the house, elicited the following reply:—

> Northleach, R.S.O., Gloucestershire,
> 24 Oct. 1902.
>
> There are no old registers to refer to, and therefore I am sorry to say I am unable to discover any entry referring to Professor Bradley. I am afraid the school is not open to inspection—it has been shut up since Xmas 1901.

For effectively altering the sense by a mere transposition of two letters, I have seldom seen anything to beat the following "printer's error" which caught my eye in reading the *Times* report, for October 9, of the recent Church Congress at Northampton. The subject of discussion was miracles, and the speaker the Rev. H. Rashdall. Since the misprint eluded the vigilance of the staff of the *Times*, attention is drawn to it by underlining two words:—

> To his mind the idea that miracles were *a priori* impossible was based upon a confusion between two totally different things—the uniformity of nature and the law of causality. The law of casuality was a necessity of thought, the uniformity of nature was not.

Some months ago reference was made in these Notes to Mr. H. G. Wells's 'First Men in the Moon'; a delightful book of imaginary travel, which maintains a high standard of scientific accuracy and probability. The other day I spent sixpence at a railway bookstall on Mr. George Griffith's 'A Honeymoon in Space,' and found it well worth the money. Mr. Griffith allows much greater freedom to his imagination than Jules Verne, H. G. Wells, or any similar writer I have yet come across. The two just mentioned are content with a trip to the Moon; and others have visited the planet Mars, as, for instance, Mr.Percy Greg in 'Across the Zodiac' (1880: but now out of print, I fear); but Mr. Griffith takes us to all the planets in turn, and with great ingenuity finds a distinct and probable character for each. Mars is inhabited by beings who have eliminated all emotion, and dispassionately try to destroy the voyagers with guns which fire poison; but Venus is a sinless Paradise, where lovely beings float in a dense atmosphere which reduces the glare of the Sun to the most perfect of temperatures. With commendable self-sacrifice the voyagers leave Venus, for fear of introducing evil into the serene community, and make their way to a satellite of Jupiter, where the inhabitants live in "glorified hot-houses" and have very nice manners. These pleasant accounts of Venus and Callisto are a welcome change from the pessimistic and dreary views of the future which such imaginative writers seem almost inevitably to adopt. But Mr. Griffith has not got up his scientific facts like Mr. Wells. He thinks the phases of a planet are due to its rotation on its own axis:—

> Its [Jupiter's] phases are invisible from the Earth owing to the enormous distance: but from the deck of the *Astronef* they had been plainly visible for some days, and, since the huge planet turns on its axis in less than ten hours, or with more than twice the speed of the Earth's rotation, the phases followed each other very rapidly. Thus at twelve o'clock noon by *Astronef* time they might have seen a gigantic rim of silver-blue over-arching the whole vault of heaven in front of them. By five o'clock it would be a hemisphere, and by five minutes to ten the vast sphere would be once more shining full-orbed upon them. By eight o'clock next morning they would find Jupiter "new" again.

The "noon" and "next morning" of this extract are also characteristic: though voyaging through space, Mr. Griffith never manages to shake himself clear of earthly trammels:—

> The Sun climbed quickly up through the star-strewn, mid-day heavens, and the full Earth sank more swiftly still behind them.

Here again is how Mr. Griffith deals with that terrible puzzle, what happens to gravitation when we travel freely in space?—

> You notice that although the wine isn't lying straight in the glass, it isn't moving about. It's just as still as it would be on Earth. That means that our centre of gravity is not exactly in line with the centre of the Earth.

These instances will suffice to show that there is plenty of amusement to be got from the book in addition to that which the author intended.

RETURNING for a moment to the statement above made, that imaginative writers nearly always take a gloomy view of other worlds and other times (especially the future); this is not confined to English writers. There are some 'Essays and Dialogues' of Giacomo Leopardi * in which there is the following conversation between the Earth and the Moon:—

---

* Translated by Charles Edwardes. Trübner & Co., 1882.

Earth. Are you inhabited?

Moon. Yes: by creatures of many different kinds, as unknown to you as yours are to me.

* * * * * * * * * * *

At least you can tell me if your inhabitants are acquainted with vices, misdeeds, misfortunes, sufferings and old age; in short, evils? Do you understand these names?

Moon. Yes, I understand these well enough, and not only the names. I am full of them, instead of the other things.

Earth. Which are the more numerous among your people, virtures or vices?

Moon. Vices, by a long way.

Earth. Does pleasure or pain predominate?

Moon. Pain is infinitely more prevalent.

* * * I firmly believe that the Sun and every star would make the same response.

This is quite as gloomy as Mr. H. G. Wells himself; and yet Leopardi has plenty of humour. One of the Dialogues gives an amusing fantasy on the origin of the Copernican Theory. One morning the First Hour goes to wake the Sun as usual and finds him so grumpy and sleepy that he actually refuses to get up and go round the Earth any longer: the Earth must rotate itself instead. The "Last Hour" must go and fetch a philosopher from the Earth to receive instructions. Off, therefore, goes the "Last Hour" to Earth and finds Copernicus wondering why the night is so long. The following conversation takes place:—

Copern. Ha! what is this that I hear? It is like the flapping of the wings of some huge bird.

L. H. Copernicus, I am the Last Hour.

Copern. The Last Hour! Well! I suppose I must be resigned. But I beg of you if possible to give me time to make my will and put my things in order before I die.

L. H. Die! What do you mean? I am not the last hour of your life.

They go off to the Sun, who explains what is wanted. Copernicus makes all sorts of difficulties: he wants a long lever to move the Earth and so on; and ultimately fears that he would be burnt alive. The Sun promises that he shall be safe if he dedicates his book to the Pope; though his disciples may not escape.—Quite an amusing little sketch altogether.

In another dialogue between Hercules and Atlas there is the same humour, rather soured, however, by the author's pessimism. Hercules comes to relieve Atlas of his burden for a time, but Atlas says it is scarcely necessary, for the Earth has become quite light and almost lifeless: instead of the strong pulsation there used to be, it only "throbs like a watch with a broken spring." Hercules and Atlas conclude that the people are all asleep; and proceed to play ball with the Earth, tossing it from one to the other, until by accident they drop it and are much frightened. But on picking it up and putting it to their ears, they hear no excitement—the people seem to be still asleep: and then comes an ingenious bit of sarcasm, to lead up to which the whole story seems to have been deliberately written:—

Hercules. In one of Horace's songs he says that the just man would stir not though the world fell. Since the world has now fallen and no one has moved, it follows that all men are just.

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